X-RAY NUMBER.

Entered as matter of the second class at the New York, N. Y. Post Office, November 26th, 1890.

VOLUME XIII. } [COPYRIGHT, 1896.] Annual Subscription $5.00
NUMBER 12. } DECEMBER, 1896. Single Copies - .50

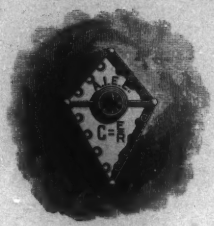
TRANSACTIONS

OF THE

AMERICAN INSTITUTE

OF

ELECTRICAL ENGINEERS.



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PUBLISHED BY THE INSTITUTE, UNDER THE SUPERVISION OF THE COMMITTEE ON PAPERS AND MEETINGS.

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AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, November 18th, 1896.

The 110th meeting of the INSTITUTE was held this date, at 12 West 31st Street, and was called to order by Vice-President Steinmetz at 8 P. M.

The Secretary announced the election of the following associate members by the Executive Committee at its meeting in the afternoon.

Name.	Address.	Endorsed by
HAMMER, EDWIN W.	Electrical Engineer, 46 Second Avenue, Newark, N. J.	W. J. Jenks. Chas. A. Terry. A. E. Kennelly.
HILL, H. P.	Engineer, Wendell and MacDuffie, 813 Havemeyer Bldg, N. Y. City; residence, Washington, D. C.	Max Osterberg. Edw. Caldwell. W. D. Weaver.
KNOX, GEO. W.	Electrical Engineer, Chicago City Railway Co., 2020 State Street, Chicago, Ill.	D. C. Jackson. C. F. Burgess. S. B. Fortenbaugh. B. J. Arnold.
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STRAUS, THEODORE.	Tester, General Electric Co., Schenectady, N. Y.; residence, 1213 Linden Avenue, Baltimore, Md.	C. P. Steinmetz. H. S. Hering. Ernst J. Berg.
VOSMAER, ALEXANDER.	Mechanical, Chemical and Electrical Engineer. The General Ozone and Electric Supply Co., Suerkade 104, The Hague, Holland.	H. Doijer. H. F. R. Hubrecht. R. W. Pope.
WALLACE, CHAS. F.	Engineer, Stone and Webster, Boston, Mass.; residence, 62 Forest Street, Roxbury, Boston, Mass.	A. M. Schoen. Chas. R. Cross. Russell Robb.
WHITING, ALLEN H.	Electrical Engineer, Riker Electric Motor Co., Brooklyn, N. Y.; residence, Stamford, Conn.	A. L. Riker. T. L. Proctor. W. L. Bliss.

WOODWARD, W. C.	Electrical Engineer, Narragansett Electric Lighting Co.: residence, 21 Arlington Avenue, Providence, R. I.	C. H. Herrick. C. D. Haskins. E. V. Henshaw.
WRIGHT, LOUIS S.	Manager, The Carbondale Traction Company, Carbondale, Penn.	S. G. Flagg, Jr. A. E. Kennelly. E. J. Houston.
YSLAS, CARLOS.	Electrician of Railways in Jalapa, Jalapa, Vera Cruz, Mexico.	C. C. Chesney. H. L. Fridenberg. Wm. Stanley.

Total 12.

TRANSFERRED FROM ASSOCIATE TO FULL MEMBERSHIP.

Approved by Board of Examiners, Oct. 14th, 1896.

NICHOLS, GEORGE P.	Partner, Geo. P. Nichols & Bro., Electrical Engineers and Contractors, Chicago, Ill.
FOSTER, SAMUEL L.	Electrical Engineer, Market Street Railway Co., San Francisco, Cal.
CUSHING, HARRY C., JR.	Electrical Inspector, Fire Underwriters' Tariff Association of New York, 32 Nassau St., New York City.
BALDWIN, BERT L.	Mechanical and Electrical Engineer, The Cincinnati Street Railway Co., Cincinnati, O.

Total 4.

The Vice-President announced that the evening would be devoted to the reading and discussion of a paper by Mr. H. Ward Leonard, entitled "Volts *vs.* Ohms." The apparatus described, had been installed upon the platform, and was shown in operation by the author, with the following preliminary remarks.

[For full text of paper, see October issue, page 375.]

Discussion.

Mr. Leonard:—It may be well for me to show the operation of the apparatus. I will say in explanation that the apparatus was not manufactured for this special purpose, and that the motor generator is not exactly of the best form for the purpose. The windings of the two armature ends are not identical, and the field strengths are not the same. The result, therefore, is not quite as good in many ways as it would be if they were very much more nearly identical. There is quite a difference; the voltage is 70 on one and 120 on the other; but I would say that it was loaned by the Crocker-Wheeler Electric Company, and will answer the purpose sufficiently well to illustrate the performance, so I will show it as well as I can under the existing conditions.

If you will turn to Fig. 4 of the paper "Volts vs. Ohms," I will point out what we have here. I have marked upon the board of the machine here the letter S, which is the shunt-wound end of the transformer; and that marked R is the reversible end of the transformer; and the motor M is connected, as you see. This is the reversing rheostat in the field of the reversible machine, which you will notice is quite large relatively to the size of this particular machine. This apparatus would be no larger, however, if it were to handle 100 kilowatts instead of one: the dimensions being due to the number of contact buttons more than anything else.

I have here a voltmeter, which is connected across the terminals of M, the motor to be driven. I will first start up the shunt-wound motor, and when running at full speed I will now adjust the rheostat in such a way that the line volts of 125 will be opposed by the 125 volts of the armature of M.

When I close the armature circuit you will see there will be no change in the current flowing, and that by the adjustment of this rheostat I will be able to make the motor armature go from rest to full speed; and not only that, but I can make it go slowly in a backward direction, for the reason that the speed of these two machines must be always equal, and as soon as the voltage of R is higher than that of S, the current will be reversed in this loop 1, 2, 3, 4; and the voltage of R being higher than the voltage of the line, M must run in a reverse direction. You will notice that there will be no change in the current when I close the armature circuit, and there will be no effect upon the armature of the motor.

By manipulating this rheostat you can make it go in either direction. By turning in this direction (illustrating), the motor will run in the direction which is the reverse from its full speed direction. In this case the E. M. F. produced by the reversible machine is higher than that of the line. By this device I can run it backward, but if the motion is to be reversed so as to run backward at full speed, it is better to have a reversing switch upon the motor armature terminals which would be thrown at a

time when there is no voltage at the terminals, and then you could go backward at full speed as the current through the armature would be reversed.

Now, there has been considerable talk as to whether or not any practical amount of energy could be restored to the line by this system, and I wish to show that this is done. This ampere meter showing the current from the line, reads both ways from zero. Now while running the motor at full speed in that direction, I instantly reverse the rheostat, thus. You noticed that when I did that, current was restored to the line. The armature reversed, and its retardation and acceleration in the opposite direction was accomplished by making the armature of M. generate useful energy which is restored to the line.

MR. G. S. DUNN:—This method of control is so perfect that it ought to be developed so as to be applicable to most of the problems we have to deal with. What has stood in its way has been high first cost. The method that Mr. Leonard has shown to-night is important because of bringing the first cost of extra or regulating apparatus down one-half. He has indicated how the cost can be still further reduced by running the motor-generator at a high speed, which is permissible because it is not in connection with any other machinery. I desire to point out that even a further reduction of cost can be made if there should be demand sufficiently great to warrant the design of special motor-dynamos.

Where control is needed, the work is usually intermittent. If advantage is taken of this fact, a motor dynamo can be designed small, whose sparking limit of load is much out of proportion to its heating limit. This will permit it to handle the currents demanded without sparking, while, on account of the intermittent character of its work, it will not become too hot. For a given output, machines of this type could be made considerably cheaper than the standard machines which now have to be used. This would bring the method of control very much closer to general usefulness than now, and I do not doubt before very long, such special motor-dynamos will be put on the market.

DR. C. T. HUTCHINSON:—I think we will all admit that the method of control exhibited here is very pretty. I am quite sure no motor armature can be controlled quite as nicely by any other method; but, as Mr. Dunn has just suggested, the whole question of its practicability is one of cost—not first cost, to my mind, but cost of operation. I doubt very much if the cost of operation of the apparatus of this kind can be brought down as low as with the more usual method of control. Every time this matter has come up before, I have taken the position that this is the least economical method of operation, and I am still of that opinion. I saw Mr. Leonard's paper a day or two ago, and to get an idea of what was being done I have examined some of the plants in the city running with this general method of control, not precisely similar, but on the same lines as Mr. Leonard's method;

and of one case in particular—that of the New York Clearing House plant—where an elevator is running on this plan, I wish to speak. Here there are four small sidewalk elevators used for bullion, and having a direct rheostat control in the armature, and one large passenger elevator with this motor generator control. To run this plant there is an engine connected directly to a large dynamo of 50 kilowatts supplying merely the power circuits, and not the lights of the building at all. A large part of the time only the one passenger elevator is running, and to run it there is the engine and large generator driving a motor, which in turn drives a generator with variable field; this in its turn drives the hoisting motor: that is, a series of five pieces of machinery—one engine and four electrical machines are used for one elevator. When I was in the building nothing was running but the passenger elevator, and the five pieces of machinery were in use to run that one elevator. The generating set has to run whenever the building is open, say from eight o'clock in the morning until six o'clock at night. That is, an engine of 75 H. P. with large generator runs 10 or 12 hours a day, most of the time doing nothing but friction work, but standing ready to supply power to the elevator whenever required. I venture to say from observations that I made, that the ratio of work done on the elevators to the waste caused by the friction of the generator turning all the time is considerably less than one-tenth; the all-day efficiency cannot possibly be 10 per cent. The claim is made for this method of control that the power required in starting the car is very much less than it is with rheostat. Judging from my observation of the same plant, this is not true; it is directly the reverse. The generator was running on a 230-volt circuit; in starting, the car required from 100 to 110 amperes, say an average of 105, at 230 volts, about 25 kilowatts, with zero volts on the hoisting motor at the start; after that, current hung in the neighborhood of 100 amperes for a little time, and gradually ran down to 40, which seemed to be about the average current for hoisting. In stopping, the current fell from 40 to zero, and reversed for a second or so. In starting down, the current went up to over 100 amperes at first, and fell gradually to about 60 where it lingered for a while and then gradually fell. On starting the elevator up, the voltage fell from 230 to 190; and on stopping, it ran up above 250; on starting the elevator down, it fell to about 180 and then ran up again to 250 on stopping. In other words, the range was from 180 to 250, or 70 volts. It is this irregularity that necessitates the separate generator for power, with another running alongside of it for lights. The power required to start the car is much greater than would be required to start the same car with rheostat control. There is no difficulty in starting such a car with a 75 per cent. increment over running power, and possibly with less. The starting and stopping of this elevator is extremely pretty; but

the speed is not high, and the acceleration is very slow, and I doubt the practical value of it.

Another case is the Fahys Building in Fulton Street. In this the plant is arranged differently; there are three elevators and three separate generating sets, each comprising an engine and generator, driving a car motor, the field of the generator being varied. Here they run from, say, eight or nine o'clock in the morning to five or six o'clock in the afternoon—three engine sets turning around idly from morning till night. Some tests of this plant have been published. These show a friction card of about five H. P., *i. e.*, about 50 H. P. hours of wasted work per day. The average total power required for hoisting was about 12 H. P., leaving 7 H. P. for the net work on the elevator. It is improbable that power is used for more than one hour daily in the aggregate: this makes 7 H. P. hours used usefully, as against 50 H. P. hours wasted,—an efficiency of 12 per cent.

Another plant on exactly the same lines is in the Sampson Building in Wall Street, where there are three separate sets, each with engine, generator and motor for three elevators.

I am sorry Mr. Leonard has not given us any results of operation. I would like very much to have him give some figures which would give some slight idea of the engineering feasibility of the thing beyond the mere illustration of the method of control.

MR. F. A. PATTISON:—I am in the same condition of mind as Dr. Hutchinson. I have always admired very much the great facility of control shown by this method, but have never been able to dissuade myself from the opinion that it is a very expensive plant to operate.

It was my good fortune to be able to compare, under very favorable circumstances, the running of the different motors with practically the same load in the Clearing House building in this city, one under Mr. Leonard's system of control—the main elevator, and the other with a load of coin said to be about the same weight, operated by the usual method. This comparison showed that the elevator operated by Mr. Leonard's system required very much more current for a round trip than the coin-lift operated under the general method.

Another plant that I was interested in was that of the *New York Herald*. But here, instead of having an engine to drive the generator, we had a motor connected with the street current. It was found that in order to cut down the expense it was advisable to have a switch inserted, so that when the elevator was not in use, this switch would be thrown and the current cut off. This was done in order to cut down the bill for simply running the machinery, that is, when it was doing no work; and to this day the elevator is operated in that way. When the elevator boy wants to use the elevator, he rings a bell and the current is thrown on in the basement, and it pays them to operate it in that way rather than to keep this machinery running while doing no work and paying the bills on the meter basis.

MR. JAMES BURKE:—I am interested in this question from a historical standpoint, and have investigated as to when this method was first used; the result may be of interest.

In 1886 a system was in use in which an Edison dynamo was used as a generator, and a 30 H. P. motor was operated therefrom, the starting and variation in speed of the motor were controlled by the strength of field of the Edison dynamo.

In 1879 a system was in use in which the speed of the motor was controlled by varying the voltage of the generator supplying this motor; the voltage of the generator in this instance was regulated by varying the speed of the engine driving it.

In 1877 a system of control was in use in which the speed of the motor was varied by operating from two generators, the voltages of which were worked in series after the motor was under way.

In 1874 a system was in use, in which 200 cells of battery were utilized for supplying current giving nearly 400 volts. The motor was started by first connecting it across a few of these cells and then gradually switching in more cells until finally the whole number was working in series.

In 1872 pumps were operated in New York by electric motors. The motors were supplied with current from batteries so arranged as to connect more or less of them in series as the conditions of operation required.

In all the instances above referred to, the speed of the motor was controlled by variations of the voltage of the supply circuit, instead of by using resistance in series with the motor. Thus the control was by voltage rather than by ohms.

MR. CHARLES P. STEINMETZ:—There are two features mentioned in this paper and inherent to the method, which have not yet been fully brought out. The one is the very gradual and easy start which may be made, irrespective of all questions of efficiency, which is of great advantage for passenger elevators—not so much for freight elevators. I remember a number of instances where this system was installed for first-class elevator service in order to get a gradual and easy start.

Another feature of the system is the ability to control the speed perfectly and to maintain it constant at a very low value, irrespective of variations of torque. This is a very important matter in certain instances, as for printing presses and so on, where it is necessary sometimes to run at very low but constant speed, which is not possible with rheostatic control, since by rheostatic control for very low speed, say 10 per cent. of full speed, the speed fluctuates excessively even for small variations of load.

Where a speed variation of only 25 per cent. or so is needed, it can generally be accomplished by controlling the motor field, but very wide ranges of speed cannot be covered thus.

In these two cases it is not so much a question of economy or

of efficiency, nor of the first investment or the cost of operation, but the necessity of securing a method of very gradual acceleration, or a constant speed irrespective of torque at a very low as well as a high value of speed.

MR. R. T. LOZIER:—In operating large printing presses one other consideration is to be taken into account, and that is the enormous starting torque that the press requires. When it is new it sometimes requires as much as five times as high as the running torque, and I do not think that with Mr. Leonard's system of control it is possible to provide for that starting torque. So for printing press work, it would be necessary to supply some mechanical method for what is known as "slow motion," and which might also be employed as leverage to overcome the heavy starting torque.

Having such device it would hardly seem practical to have an independent engine and generator and throw a load on the engine of 3 or 400 per cent. momentarily—supposing that this starting torque is necessary for that purpose—it would seem that this mechanical slow motion might be accomplished at a ratio of 20 to one. Then some form of rheostatic control in conjunction with that slow motion would seem to serve the purpose.

If it were not for the high starting torque I should say that the Leonard system for large printing presses was an ideal method; providing, of course, people are willing to pay the additional first cost, because the printing presses when once started run at full normal load, and the generating apparatus is again working at its highest efficiency.

One thing I have found in printing press work, and that is that all the presses will strike a mean load, so that taking the normal rating of the motors you will find that the load on the generators is fairly constant at perhaps 60 per cent. of the power required, if all the presses were running at once at their full capacity.

MR. WILLIAM ELMER, JR.:—It strikes me that the last speaker has confused starting torque with H. P. I do not think the H. P. required from the engine is anything like the amount which he seems to have in his mind. The starting current may be large, but if the voltage is small, the power is also small.

MR. R. T. LOZIER:—Hoe & Co. are unable to start a sextuple press, if I am not mistaken, with a 12-inch belt, but they first start it through their slow motion mechanism running with a five or six inch belt. The 12-inch belt drives the main shaft 200 revolutions; the slow motion is 10 revolutions. The long train of gears, ink rollers and the type plates setting down on the blankets form an enormous resistance to overcome in starting. The starting torque is of short duration, really a peak if you should diagram the load.

I know of one instance where the Leonard system was used on printing presses and abandoned on account of the excessive starting loads of the press. Rheostatic control is now used in

that particular instance, with a separate belting system, and operating through mechanical slow motion.

MR. LEONARD:—In reply to what has been said, I will say it is no doubt true that conditions can be obtained under which this system would be absolutely worthless. These conditions exist in such a plant as that of the *New York Herald* which has been cited. They apparently also exist in the case of the New York Clearing House, also referred to. To take an elevator that is to be operated once or twice a day, and that has no desirability of high speed nor control, and with very short lift, such as the *Herald* which is extremely short, and to attempt to apply a method whose chief virtue, if it has one, is the susceptibility of perfect control at high speed, is of course, creating conditions which are the very best possible to make the method worthless. I do not know anything about the Clearing House elevator, for I never saw it. For instance, if the motor generator is installed for the purpose of running an elevator in a low building, such as the *Herald* building, it will almost invariably be the case, unless the system has such advantages as regards the perfection of operation and control and freedom from accidents and repairs, as to make it of comparatively slight importance whether ten or fifteen dollars more per month is paid for coal, that no such motor generator should be installed for running an elevator.

In a large office building it is always more economical to produce your own electricity for lighting by an isolated plant, and when a plant is installed for that purpose it should be used for the elevators, but not with separate engine for each particular generator, with cut-off varying from zero to full load of the engine. That is such bad engineering that you cannot expect to gain results from any system under such conditions. But if an office building be equipped with this method of control for the elevators, with perhaps two engines so arranged as to have directly coupled between them, on a shaft joining them, the necessary generators to take care of the lighting and elevators; for example, one generator for lighting and perhaps three for the elevators, which would be a common plant; four generator armatures mounted on one shaft with an engine on each end and with a coupling between the engine and shaft at each end and perhaps a coupling in the centre of the shaft; the lighting generator supplies the necessary current for exciting the fields of the elevator generators and for the field of the motors driving the elevators, and you are ordinaily producing your energy then in one cylinder, and your other engine can be made available to work in conjunction with the first, or you can split your plant in the middle and have two generators on each engine. Under conditions of that character this system would show most valuable results. The best indications of what results may be obtained if the load upon the engine be reasonably constant, is indicated by the fact that in the case of the elevators that have been cited in the Fahys Building, in which there is an individual

engine for each individual generator, the maximum power in starting is about 12 H. P., and the running H. P. at full speed is in the neighborhood of about 15 H. P.; the coal bill for a month for running three elevators and for all the lighting in the building is 40 tons of pea coal per month. That figure I think will compare favorably with the lighting and elevator service in any other building in the city, and yet the conditions of the use of the method of control for the elevators are extremely unfavorable.

The figures which Dr. Hutchinson cited I cannot answer, because they appear to me to be due to such unusual conditions, that there is some explanation for, which can only be known to himself. The idea of a generator being run by this system from 180 volts up to 250 volts is simply due to conditions that are in some way ridiculous and are no part at all of this method or system, and have nothing at all to do with it. I do not know the plant that he speaks of, and I do not know the causes leading to the results which he has stated, if they be as he has stated them. But, of course, this thing has been discussed before, and we know his views in regard to this system and he knows mine, and not much can be gained by discussing the matter between us any farther. But in the plant that was tested in the Fahys Building the figure of that test as to the kilowatt hours per car mile run was the best figure that has ever been obtained yet, so far as I know, and yet the conditions were unfavorable.

As to the historical figures which have been given by Mr. Burke, most of them of course, are well known facts that are entirely independent of, and have no relevancy whatever in regard to this method so far as I know. This method does not claim to be the only thing that was ever done in the way of varying volts, but it is limited to peculiar methods and peculiar combinations, all of which are radically different from those which he describes.

Mr. Lozier has evidently a very confused idea in regard to the question of torque and power. If there is any one thing this method will do it is to create an enormous torque with a very little power. That is its chief claim; and this armature here can be run so that it will hardly turn over and yet you cannot hold it with your hands, and it will be taking a very few watts to do it. The printing presses he describes are not as bad as he thinks, and when this method was first installed in 1891 it was upon presses which required 30 H. P. to run each of them for printing calico; and yet when all other methods by a rheostat control and other methods of that nature had utterly failed because of the enormous power required, I installed this, and started the presses invariably on a voltage of about 20 volts on an armature of a machine which was designed for 125 volts and with a current which was probably four or five times the current of full torque, because the men that were running the presses were not favorably disposed to the system. They have seen them monkeyed with for about a year with electric motors, and everything had failed, and when I succeeded

in starting the press they not only put on the press the maximum torque which could be obtained under normal conditions, but they screwed the rolls down to such a degree trying to stop it, that the owner of the plant who came in at the time almost discharged the foreman on the ground that he had sprung the frames of his machine.

As to the size of the belt, that does not enable you to determine the power. The belt is merely the measure of the torque. My belt in that instance was about 12 inches, and I had to get it as tight as a fiddle-string, because my motor would start up every time and the belt would slip, and yet I was using only about four H. P. Of course, a belt running something like 4,000 feet per minute can transmit a great deal of power, but when running slow will do very little as regards power. All that the size of the belt will give us is torque. The power is proportional to the speed of its movement.

As to the printing press which is said to have been operated by my method in this city and abandoned, I have no knowledge. I do not know where it is or what it is, and I have not heard anything of the sort myself. But all I can say is, that if it was abandoned solely because it could not get enough torque to start the press, there must have been somebody handling it that did not understand the method, because there is no doubt about starting anything that the armature will start under. The torque is practically unlimited because you have an armature starting from dead rest with a current which can be increased to a current represented by the ohms of the armature divided into the entire volts of the line, and nothing in practice ever requires such a torque as that.

As regards the operating cost of the method and the first cost of the apparatus, I do not for a moment grant that under fair conditions this is not the most economical plant to run, but I do want to point out that there are a great many cases in which the first cost, and the cost of maintenance are not the considerations which determine the choice of the apparatus. For instance, this method has been used for the handling of the turrets on the men-of-war. If anybody thinks that turrets can be satisfactorily handled by any conceivable system of rheostat control so as to enable the guns to hit anything when you get ready to shoot, all he has to do is to try something of that kind or watch others try it to learn how absolutely impossible it is to have perfect control by rheostats when you have a motor which is hardly moving. When you have to start up under the inertia and sticking of the parts, the torque will be quite high and a large portion of the rheostat will have to be cut out to get enough current to start it. The minute it is started, the torque will be about one-quarter of what it was and then the motor will run like a scared cat. In this particular case rheostat control was not in question; the question was simply of control by steam as against control by

electricity. My method of control was used as representing the possibilities of electrical control, and the steam machinery was the result of a large number of the best engineers of the country working out the most refined methods for handling things of that kind. It is of very little use to have a million dollar gun with its platform, if you cannot hit anything when you shoot. The control is of prime importance, and the question whether the method costs $100 or $500 more to install is of no consequence, and the question whether it costs a few more pounds of coal to run it (which we will assume to be the fact, but which I do not grant at all, understand me) does not make any difference in the result if they can shoot and hit something every time they shoot because of the perfect control, or if they can shoot more frequently. This method was tested, and it was found that they could make 23 distinct starts and stops of the gun under perfect control within a period of 20 seconds, and not move the gun one degree. The steam men after spending a long time getting every possible refinement of their method, finally were able to accomplish such a thing as that, by backing and filling over the line for five minutes, while by that time the object they were going to shoot at would be beyond the horizon. That is one case where the question of first cost and economy of operation are of no importance as compared with the question of control.

Another case that I have already spoken of is the question of operating printing presses of large size. The persons who have had these printing presses running with this method since 1891 would have nothing else for their presses, although every other method that was conceivable to the General Electric Company was worked upon and developed to the highest degree, but this method was installed because nothing else would do the work satisfactorily. Similarly in case of another plant that is at this time going into operation, which is to have the same method of control for the same purpose. The system is also used abroad for the handling of cloth printing presses, and I think I am safe in saying that no other method is so used. Printing presses of the ordinary kind, where the question of control is less important than in the case of printing cloth, are being run by rheostats, but in a very unsatisfactory manner. I am in the rheostat business myself and will be delighted to build them for anybody and everybody who likes them. A big rheostat is bad enough where the movement of the lever is always in one direction cutting out resistance, but for regulating the speed of a motor by a rheostat where the resistance is to be inserted, there is so much sparking and such a perfect lack of control that it only means that the person using it is not familiar with the possibilities to be accomplished by such a method as this. Of course, if you have a shunt-wound motor and then have some kind of a mechanical reduction of power between that motor and the press, that is all right, and it makes a first class system which will give most satisfactory re-

sults; but the difficulty is, that that mechanical gearing is the thing that all mechanics have been hunting for for fifty years and have not found yet, and it looks as though, for big powers, it is about as far off as when they started.

Dr. Hutchinson has asked whether I cannot give some figures with regard to the results of operation of this method of control. You will understand that this is the first time this new system has been described by me, and it has not been much used. Two or three companies have made use of it, but I have no figures at hand bearing upon the question of economy as yet, but since the system so far as the public are concerned is absolutely new, it is not to be wondered at that the figures are not thus far available. But I will point out this; that when this motor under this system is running at full speed only one-half of the energy it uses is converted, and the other half is supplied directly from the line; when at half-speed, no energy is converted; when running at lowest speed, it is true that nearly all the energy in the motor armature is converted—perhaps one-tenth or one-twentieth of the current comes directly from the line, and the balance comes from s acting as a generator, s being driven by this other machine R acting as a motor. But I think it will not be doubted that the loss in the transformation of that energy cannot compare with the loss in the case of a rheostat, which is about nine-tenths of your total energy.

Dr. Hutchinson:—Mr. Leonard abandons all the elevators operated by his system which he has formerly described here, including the Fahys Building; the New York Clearing House he has never seen; it is of course abominable engineering.

Mr. Leonard in his remarks, though he does not say so, seems to imply some peculiar virtue in the increase of the torque of the driving motor. You can get just as great torque by starting directly from the line as you can by that system. In one case you overload the line generator, and in the other case you overload the machine s. It is the same degree of overloading, and the only question is of line drop. There is no peculiar virtue by which the driving torque can be multiplied by this arrangement. As I understand his position at present, he does not make any special claim for economy in the system. Practically he admits that economy has nothing to do with the question, but that where ease of manipulation is essential or to be desired, that this system should have the preference over any other. In that I agree with him thoroughly.

A comparison of the energy expended in moving a ton a mile on an elevator and on a tramway may be interesting; for an elevator of about 25 square feet platform area, running about 250 feet to the minute, it requires about 2000 watt-hours per ton mile; an ordinary tramcar requires about 300 watt-hours per ton mile. This is due, of course, to the average 50 per cent. grade of the elevator car.

MR. LEONARD:—I am not surprised that Dr. Hutchinson has retreated from the position which he assumed a few months ago when he made comparisons before us on the basis of kilowatt hours per car mile. The reason is because of the favorable showing of the elevators in the Fahys Building, where tests showed a power required of only about 60 or 70 per cent. of the most favorable figures he claimed. That, of course, makes it necessary for him to change the basis of figuring. He knows too well, and I hope none of you will be misled by his statement as to torque. I made no claim of securing a greater torque than can be obtained directly from the line. What I claim is, that this system secures a very large torque from a constant potential system with very small amount of power, which cannot be done by any rheostat system. Of course, you can get just as much torque from a constant potential system with rheostat control. It is purely a question of current through the motor armature; but I will produce that current for starting the motor with one-twentieth part of the energy required in the case of rheostat.

He said the only consideration was the drop in the line. The drop in the line, while important, is of comparatively slight importance; the power used and the control are the main factors.

Another point which has not been mentioned particularly is the question of restoration of energy to the line. Now, I cannot give any figures for this, except as regards elevators, and as to elevators, the system has been used but comparatively little and then not under the best conditions; but I want to remind you that one of the oldest electrical concerns in the world and one proven to have the best judgment—the Siemens and Halske Company—are putting in plants abroad, employing shunt-wound motors for the reason that such a motor has the power to restore some energy to the line. This is a very important matter, and, in the case of this system, can be brought to the very highest degree of perfection. Of course, in the operation of this motor if you are on long runs, without stopping or starting, the transformer would be entirely cut out of circuit and not in service at all, if you did not need the boosted volts for the full speed.

Another point I have not mentioned, but which will throw some light on the question of first cost, is this:

The variation in speed of motors to-day is accomplished to a certain degree by the variation in the field, and all that can be done in that way can be accomplished as an additional feature for this method. By weakening the field of S, this machine M will go faster with regular normal fields, and then by weakening the field of M we can go still faster. So that, if we have to run at high speed on a level, where the torque is comparatively small, we can make use of an apparatus which will be even smaller than I have hinted at in the paper, by reason of the fact that the current can be held within sparking limits even in the weaker fields and yet give enough torque for the high speed service on the level.

MR. CARICHOFF:—I wish to cite another example where the current produced by shunt wound motors, may be restored to the system. In the Siegel-Cooper building there are 16 passenger elevators, capable of carrying from 40 to 50 people each. They are geared to carry 5,500 pounds at 150 feet per minute. The current required to carry 5,000 pounds net on the hoisting ropes of one of these elevators, if I remember rightly, is 110 amperes; the same machine on the down trip will restore 60 amperes, and as all of the elevators are connected electrically, one machine ascending is assisted by some other machine descending.

Half the machines going up and half coming down at the same time, require 25 amperes average per machine, each with 5,000 pounds net on ropes. In fact this whole plant consisting of 16 passenger elevators and five sidewalk elevators, runs with less than 400 amperes of current at 230 volts.

The elevators run about 150 feet a minute, and calculated on above basis the energy required is 3.4 kilowatt hours per car mile of travel; if these elevators were carrying 2,500 pound net, and running 300 feet, they would require only 1.7 kilowatt hours per car mile.

Similarly the same number of foot pounds, viz: $150 \times 5{,}000$ or 75,000, may be resolved on a properly geared machine into 1875 pounds at 400 feet per minute, and the energy per car mile becomes $3.4 \times \frac{150}{400}$ or 1.27 kilowatts per car mile of travel.

Again, if the net load is one half of 1875, or about 900 pounds on the ropes, as was given in the reported test of the elevators in the Fahys building, the energy per car mile of travel becomes one-half of 1.27 or 0.635 kilowatts per car mile of travel.

The above inferences are based on continuous running with constant load. Starting and stopping, and changing load, will of course introduce modifications.

Allowing 50 per cent. increase for frequent starting, we still have less than one kilowatt per car mile of travel for a group of rheostat controlled elevators, or for one or more with storage battery, operating at same load and speed as those in the Fahys building, against 2.7 kilowatt hours per car mile as reported in a test of that plant, for a *combination that does not waste energy in resistance.* It must be borne in mind that the load here considered is the net pounds on the hoisting ropes; the weight carried in the car may be more or less than this net load according as the system is over-counterweighted or under-counterweighted.

MR. LEONARD:—The test of the elevators in the Fahys Building showed 2.7 kilowatts per car mile; but, of course, what figure we might get is one of those things we can all guess at. I think that the last speaker said 1.7, well, I guess that with my system and with the 16 elevators all driven by one common source of power the figure would be cut down to 1.2.

DR. HUTCHINSON:—Mr. Leonard ignores the fact, in claiming

that he gets his torque with less power at the main machines, that in the case I cited, the New York Clearing House, this is not true. He brushes aside my figures, by saying something was wrong,—in which I agree with him; it is his system. I repeat, at this plant the starting current is over 100 amperes, at 230 volts, for a car having about 25 square feet floor area, running empty and accelerating slowly.

Mr. R. T. Lozier:—Mr. Leonard is of course quite correct about the starting torque not representing the H. P. obtained by multiplying the amperes taken in starting, and full voltage at terminals when motor is running at full speed. I have observed with him both the amperes and volts on one of these large presses at starting and when running at full speed.

I think that every manufacturer of dynamos and motors is in favor of the Leonard system, because by its installation we sell more dynamos and motors—provided we can get them in. Let me state that in the instance cited where his system was used and abandoned, I did not make the trial.

Mr. Leonard:—About that system that was installed and given up; that rankles a little bit; I don't know where it is or what it is, but if it is the printing press which I rather fancy it to be—and I think I have some confidential information which enables me to locate it—it is a case in which there was used a generator built some—20 years ago I was going to say, but at any rate a great many years ago—of a type that is obsolete, and not one that was well adapted to large fluctuating currents. It has a very small commutator, the type of commutator in vogue in 1885. It was not a generator such as would be suitable for running any kind of elevator or printing press, if it is the case I think of: and, it may be, the fact that the old generator was used for the purpose, rather than a generator of modern design, will account for its not being satisfactory and therefore abandoned.

[Adjourned.]

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, December 16th, 1896.

The 111th meeting of the INSTITUTE was held this date at 12, West 31st street, and was called to order by President Duncan at 8 P. M.

The Secretary announced the election of the following associate members by the Executive Committee at the meeting in the afternoon.

Name.	Address.	Endorsed by.
ADAE, CHAS. FLAMEN.	X-Ray Laboratory, P. O. Box, 2809; residence, 36 West 35th Street, New York City.	A. L. Riker. T. L. Proctor. W. L. Bliss.
BLUNT, WILLIAM W.	Electrical Engineer, Westinghouse Electric Co., Ltd., 32 Victoria St., St., London, Eng.	Chas. F. Scott. A. J. Wurts. C. A. Bragg.
BYRNS, ROBERT A.	98 Ferry Street, Lafayette, Ind.	H. B. Smith. E. F. Norton. W. E. Goldsborough.
CRAIN, JOHN JAY.	Electrician's Helper, Niagara Falls Power Co., Niagara Falls, N. Y.	Edw. L. Nichols. Harris J. Ryan. C. P. Matthews.
HUMPHREY, HENRY H.	Consulting Electrical Engineer, Bryan & Humphrey, Turner Building, St. Louis, Mo.	F. G. Schlosser. J. E. Randall. W. F. White.
KITTLER, DR. ERASMUS.	Elektrotechnisches Institute, Darmstadt, Germany.	Carl Hering. Ralph W. Pope. A. A. Knudson.
LATHAM, HARRY MILTON.	Member of Engineering Staff, Crocker-Wheeler Electric Co., Ampere, N. J.	S. S. Wheeler. Gano S. Dunn. F. M. Pedersen.
STEWART, ROBERT STUART.	Supt. of Lines Public Lighting Commission, 440 Jefferson Ave., Detroit, Michigan.	Alex Dow. C. F. Brackett. Jesse M. Smith.
SUTTON, FRANK.	Consulting Engineer, 27 Thames Street, New York City.	M. I. Pupin. F. B. Crocker. Max Osterberg.

Total 9.

TRANSFERRED FROM ASSOCIATE TO FULL MEMBERSHIP.

Approved by Board of Examiners, November 11th, 1896.

LOOMIS, O. P.	Electrical Engineer, Bound Brook, N. J.
SCHOEN, A. M.	Electrician, So. Eastern Tariff Association, Atlanta, Ga.
FIELD, H. G.	Consulting Electrical Engineer, Detroit, Mich.
MCMEEN, S. G.	Engineer, Central Union Telephone Co., Chicago, Ill.
MCCROSKY, J. W.	Electrical Engineer, La Capital Tramway Co., Buenos Aires.
FORTENBAUGH, S. B.	Ass't Prof. of Electrical Engineering, University of Wisconsin, Madison, Wis.
PARKER, LEE HAMILTON.	Ass't Engineer, Railway Dept. General Electric Co., Schenectady, N. Y.
BRINCKERHOFF, H. M.	Electrical Engineer, Metropolitan West Side Elevated R. R., Chicago, Ill.

Total 8.

THE PRESIDENT:—The subject for discussion this evening, gentlemen, is "The Röntgen Ray, and its Relation to Physics," and the discussion is to be opened by Professor Rowland.

A Topical Discussion at the 111th Meeting of the American Institute of Electrical Engineers, New York, December 10th, 1896.

THE RÖNTGEN RAY, AND ITS RELATION TO PHYSICS.

(A Topical Discussion)

Opening Remarks by Prof. Henry A. Rowland.

Mr. President and gentlemen: A gentleman asked me a few moments ago if I knew anything about the X-ray. I told him no; that what I was going to tell to-night was what I did not know about the X-ray. I do not suppose anybody can do any more than that, because all of us know very little about it. We were very much surprised, something like a year ago, by this very great discovery. But I cannot say that we know very much more about it now than we did then. The whole world seems to have been working on it for all this time without having discovered very much more with respect to it.

Now, I suppose it is not necessary for me to go into the history of the thing. We all know it; how Lenard first, probably, discovered these rays, or discovered something very similar to them; how Röntgen afterwards found their particular use, their penetrating power, and so on, although Lenard had found something similar to that before. It is thus not necessary for me to go into the history of the matter, but simply to go over, to some extent, what we know with regard to these rays at the present time. First, there was some discussion, some time ago, as to the source of these rays. Röntgen found that their source was any point that the cathode rays struck upon; and you will remember that when we first knew about these rays they were often called cathode rays. Many persons thought that the cathode rays came through the glass, and Lenard first thought that they did come through his little window, and it is probable that they do at the present time. But the kind of rays that we are considering are very different from the cathode rays. Six months ago there was quite a discussion in regard to the source, and I believe it was finally determined that they came from points where the cathode rays strike. At the same time I was rather opposed to that. In one of my tubes I found that the rays came from the anode. I had

only the ordinary assortment of Crookes tubes, and one of the tubes had aluminium wires which were a millimeter apart. In one of these the source of the rays was a point upon the anode—not upon the cathode at all. It was a very small point. The photographs which I obtained by that tube were sharper than any I had seen before. They are so very sharp that in estimating the shadow of an object I determined that the point could not have been a thousandth of an inch in diameter. Therefore the source in this case was a very minute point upon the anode, and that point was nearer the cathode, and I suppose some of the cathode rays might have struck upon it, and it might have obeyed the law that the point where these X-rays are formed is the point on the anode where the cathode rays strike.

I had another very interesting tube, and I was going to bring some of the photographs here to-night; but I thought they were so small that it would be almost impossible to see them. I tried the three cases in this tube: First, the case where the cathode rays strike upon the anode. In that case I got very many Röntgen rays. Then I tried the case where the cathode rays strike upon an object—a piece of platinum. I did not get any rays whatever then. Now, some people say that they come from the point where the cathode ray strikes. I did not get any whatever in that case. In this case the cathode ray struck upon a piece of platinum in the centre of a bulb, and no rays were given out by the anode either. Therefore I seemed to have a crucial experiment in each; I seemed to have the case where the cathode ray strikes upon the anode, and I got lots of rays. Then I had the case where the cathode rays strike on a piece of platinum, and I did not get anything at all. Then where the anode itself was free and no cathode rays struck it, I did not get anything from it. It seemed to me as if the source was most abundant when the cathode ray struck upon the anode; and that is the theory, we know, upon which nearly all tubes are formed at the present time. You have the focus tubes in which you focus the cathode rays upon the anode, and in that case you have a very abundant source of ray; but I do not believe you ever could get as small a source of rays as I got with that first tube, where I had a source of a thousandth of an inch diameter. Having such a small source of rays, it gave me a limit to the wave length, if there were waves at all; it would give me a limit to the wave length of which I will speak in a moment. As to whether there are any rays where the cathode rays strike on any other objects, we know that there are very feeble ones It seems to be almost necessary in order to get an abundant source that you should have cathode rays strike on the anode. However, that is a point of discussion. Now, as to the source of electricity, we have generally the Ruhmkorff coil. There is one source of which I saw a little note in *Nature*, where a man had used a large Holtz machine with very good effects. Now it is

very much easier for many persons to use a Holtz machine than to use a Ruhmkorff coil. There are many cases where one cannot have a large battery; and this man said that with the Holtz machine he got as great an effect as with the Ruhmkoff coil. Then we have the Tesla coil, etc. By the way, speaking of the Tesla coil, I am not sure but that you might look back and find that it is very similar to the Henry coil. Henry originally experimented on the induction of electricity, transmitting a spark of electricity from one coil and getting a spark from another, and the Tesla coil is something like that, except that it is made so as to produce a much more voluminous spark.

We all know the properties of the Röntgen rays—they go in a straight line. Every effort to deviate them from a straight line, by any means whatever, has failed, except that when they strike upon an object they are reflected. Now, it is a question for discussion as to whether there is any regular reflection. They strike upon an object, and you get something from that object which will affect a photographic plate. Are those rays which you get from the object Röntgen rays still, or do the Röntgen rays strike upon this object and generate in it some sort of rays which come out, different from the Röntgen rays, and affect the plate? We do not know that. Neither are we quite positive whether there is any reflection of the rays. We know there is turbid reflection—you may call it—rays strike on the object, and the object becomes a source of rays of some kind. Nobody has ever found out what sort of rays come from the object. Something comes from it, and we generally imagine, and indeed we often state, that they are Röntgen rays that come off the object. But we have good reason to suppose that they may be something else; and they may or may not be regular reflections; some persons say they are and some that they are not. I have seen some photographs made in this city which indicated regular reflections. At the same time I would not be positive as to whether there was any regular reflection. It is rather doubtful. It is a point to be determined.

Then the fluorescence—that is the way Röntgen originally found the ray. You know the way they produce fluorescence—the photographic effect—you all know that. You all know that the magnet does not affect them—does not turn these rays from a straight line.

The polarization of the rays: We have no evidence whatever as to the polarization. If they were very small waves, transverse waves, like light, we ought to be able to polarize them. Becquerel, by exposing certain phosphorescent substances to the sun, obtained from them certain rays which penetrated objects like aluminium, etc. But these rays were evidently small rays of light, because he could polarize them, and he could retract them, and they were probably very short waves of ultra-violet light. But we never have been able to discover that

there was any such effect in a Röntgen ray. Some persons have claimed that they got polarization; but if there ever was any polarization, it is very small, indeed One of the principal advances in respect to these rays is that made by J. J. Thomson, in considering the electric discharge of bodies. He has published most valuable results with regard to the effect of these rays upon gases. When the rays fall upon a gas, they affect the gas in some way so that it becomes a conductor. Now, you can subject the gas to these rays and allow the gas to go through a tube off into another vessel, so that it will discharge an electrified body in that vessel. But he has found the most interesting result that it will not continue long to affect these bodies. After one has allowed a certain amount of electricity to pass through it, it then becomes an insulator again. It only allows a certain amount of electricity to go through it. That is easily explained—or you can explain it—by the Röntgen rays liberating the ions, and only a certain amount of them. Just as soon as these are used up in the conduction of the gas, then it ceases to conduct. So that a certain amount of gas will conduct a certain amount of electricity, and then it stops conducting. That is a most interesting result. It is one of the great advances we have made since Röntgen's discovery. Röntgen knew nearly all we know now about these rays. We have discovered very little indeed; but that point I think we have at least discovered.

Then it is said that these rays affect a selenite cell in the same way that light affects it—it changes the resistance of the selenite cell.

Of course, we are only considering the theory to-night; at least I am, and we do not have to consider the bones, and so on. I have had some students at work in my laboratory, and it was with the utmost difficulty that I kept them from photographing bones. Bones seemed to be the principal object to be photographed by the Röntgen rays when they were first discovered, and I suppose it is the same now. Most people connect Röntgen rays with bones; but I do not intend to say very much about them.

Now, one important point with respect to these rays is as to whether they are homogeneous. Are they like light which can be divided up into a large number of different wave lengths, or are they homogeneous? There seems to be a great deal of evidence that they are not all the same; that one ought to get a spectrum of them in some way. We can filter them a little bit through objects. After they are filtered through an object, they are probably a little different from what they were before, and some objects probably let through different rays from others. In *Nature* Mr. Porter, I believe, has shown experiments upon that. He divides rays into three kinds. At least he finds that under certain circumstances the rays will penetrate bones better than in other cases—bones or any other object—they have more

penetrating power, and they go through many of those objects that ordinarily stop them. By heating up the tube, and by various arrangements of his spark-gaps, etc., and putting little wires around his tubes, and so on, he can cause them to generate different kinds of rays. That is a very important point, if it is substantiated, and there seems to be little reason to doubt that a number of rays really do exist; that whatever they are that come from the object, they are not all the same; some of them penetrate bodies better than others, and very likely some one will get up some sort of filter that will filter them out, and allow us to use them and to find if they have different properties. At the present we are rather in the dark with regard to this point.

Now I come to the theory of these rays. What is the cause of all these phenomena? There was a time when we were rather self-satisfied, I think, with regard to theories of light. We thought that Fourier and others had discovered what light was—some sort of vibration in the ether; we called it ether; if it had these waves going through it, then it would produce light, and we were pretty well convinced that the waves were transverse, because we would polarize them; so that we began to be satisfied that we knew something about light. Then Maxwell was born, and he proved that these rays were electro-magnetic—very nearly proved it. Then Hertz came along and actually showed us how to experiment with these Maxwell waves, most of which were longer than those of light. At the same time they were of the same nature. Well, we got a rather complicated sort of ether by that time. The ether had to do lots of things. One must put upon the ether all the communication between bodies. For instance, what communication is there between this earth and the sun? Why, you have light coming from it and heat. Radiation you might call it all. We have radiation. Then some people thought they discovered electro-magnetic disturbance from the sun. Sometimes they have seen a sun spot and noted a deflection of the magnetic needle on the earth. Very likely that is true. I don't know that they have discovered any electrostatic effect. But we know that electrostatic effects will be carried on through as perfect a vacuum as you can get. Then we have gravitation action too. Now, you have got all those things—electro-magnetic action, light which would be an electro-magnetic phenomenon, and then we have gravitation, and we have got to load the ether with all those things. Then we have got to put matter in the ether and have got to get some connnection between the matter and the ether. By that time one's mind is in a whirl, and we give it up.

Now we have got something worse yet—we have got Röntgen rays on top of all that. Here is something that goes through the ether, and it not only goes through the ether but shoots in a straight line right through a body. Now, what sort of earthly thing can that be? A body will stop light or do

something to it as it goes through; but what on earth can it be that goes through matter in a straight line? Why, our imagination doesn't give us any chance to do anything with that problem. It is a most wonderful phenomenon. Now, we can suppose that they are ultra-violet light. Indeed, we can get a limit to the wave length to some extent. Nobody, however, has ever proved that the Röntgen rays are waves. But we can get a limit of the wave length if they are waves, because when I have a tube that gives me a shadow which is only a thousandth of an inch broad, or rather from the greatest intensity out to clear glass a thousandth of an inch broad, I can calculate the wave length of the thing that would produce such a shadow. It has got to be very small indeed; one knows that right away, because any ordinary light would make a few waves at the edge of the shadow, and by measuring those waves you could get the wave lengths of the light. But there was no appearance whatever on any of my photographs of any such phenomenon as that. I did not have any of these waves at the edge of the shadow whatever. It went directly from blackness to light. But putting it under the microscope and measuring from almost imaginary points, from lightness to darkness, I could get a limit to the wave length. Now, as to that limit, I published it in one of the journals six months ago, or more, and it came at about one-seventh, I think, that of yellow light. Others have determined the wave length and got even below one-seventh that of yellow light. Some have got one-thirtieth that of yellow light, and so on. Some of them I am rather doubtful about, because they say they have bands. If they have bands and defraction bands, that would prove instantly that the Röntgen rays are waves. But I have never seen the slightest phenomenon of that sort. It is very doubtful that it exists, and those persons who have had it will have to show their photographs very clearly to make us believe it. And therefore we have no evidence whatever that the rays are waves. At the same time we have no evidence that they are not waves. They might be very short waves—infinitely short waves. Let us see what would happen if they were infinitely short waves. They might be so very short as to be too fine-grained for any of our methods of polarization or reflection. Waves are reflected from a solid body—regularly reflected, because they interfere after they come from the body. You can get the direction—the angle of incidence equals the angle of reflection; you can get that by means of considering them as waves and as interfering after they come from the object. Well, if the object, however, is a very rough sort of thing compared with the wave length, you will not get a regular reflection. That is what might happen in the case of Röntgen rays. And then again, with regard to refraction of the light, the theory of refraction which comes from considering molecules imbedded in the ether will give you some limit. When we go beyond that limit, we get no refraction.

The bending of the violet rays increases up to a certain point and then goes back. We have a case of anomalous refraction very often in some substances like fuchsine, aniline dyes, and so on. Therefore the action of refraction can be accounted for by having very short waves. But when we treat of the theory of the case we have the little molecules of a gas knocking against each other, and they can only go a little distance. We call that the free path of the gas—a very small distance in the ordinary air. Those molecules cannot go more than this very small distance before they stop. Well, now, why should little, short waves of light pass through the gas and not be stopped too? When the waves are very short indeed, it seems to me that the object would be entirely opaque to them, because they would strike upon those molecules, unless they could pass directly through the molecules. You would therefore necessarily have these little short waves going directly through the molecules, which we generally think is almost impossible in case of light. And that is one very great objection that I have to that theory.

Then we have another theory—that these are not transverse waves at all; that they are waves like sound, and very short indeed. Well, what would happen then? If they are very short indeed, you have the same objection: They would all strike against the molecules, and they would be dispersed very quickly. The shorter the wave lengths, the more they are dispersed. Take, for instance, short waves that bob against a boat and are reflected back. Thus, if you have a big, long ocean wave, it sweeps around a boat and goes on without being troubled by the boat at all. The shorter the waves, the more they are bothered by the boat, and so it is with respect to other waves—the short waves would probably be stopped by the molecules. So I do not see what we can do with regard to it in that respect. According to Maxwell's law, waves like sound do not exist in the kind of ether that he suggested. But that is all based upon a certain theory that the lines of force were always closed. He introduced into his equation an expression which indicated that every line of force was a closed path coming back upon itself or ending in electricity, one or the other. Now, if we throw out that, then we can get this kind of compressional waves in the ether. Now, it is not at all impossible that they exist, and as to whether they would go through molecules any better than light waves do, nobody can tell; but it is possible that they might. But if there are waves at all, they must be very short waves. You cannot get over that fact—if they are waves at all, they must be short.

Then, of course, you have the other theory—of little particles of matter flying out from the body, passing through the glass and all other bodies, until they reach a photographic plate or any other place where we are notified of their presence, and these little particles make their way through the air or any other substance. Now, why should not the little particles be stopped

very quickly by bodies as well as if the rays were waves? You see we are in trouble here too. Why are not the waves stopped? Why are not the little particles stopped? Stokes has given some sort of a theory with regard to this—that, instead of having a wave motion in the ether, the rays are impulses—a sudden impulse—one wave, for instance—not a series of waves at all, but one impulse coming out from the tube. I think if he had seen any very sharp shadows obtained from the Röntgen rays he would not have given that theory. He probably has seen only those very hazy outlines that very many persons take for Röntgen photographs. But if he had seen any very defined ones—very sharp ones—he probably would not have given that theory, because if the Röntgen rays are waves at all, they must be short, and there must be a long series of them to make sharp shadows. This is why Newton gave up the wave theory of light. You remember he gave up this theory because he found that light went straight past an object instead of curving around into the shadow as much as sound does. But he was not quite up to his usual pitch when he made that statement, because if he had thought a moment he would have seen that very short waves will go more nearly in a straight line than long ones. But any single impulse, such as Stokes suggests, would go into the shadow. The only wave motion that would go in a straight line is a series of waves, one after another. Therefore, these rays cannot be single impulses coming irregularly.

Prof. Michaelson has suggested a theory of rays based on something like vortex rings in the ether. Now, if we have an ether that can carry on light waves and electro-magnetic waves, it cannot be a perfect fluid; it has got to be something else. You cannot very well imagine vortex rings in such an ether. So that we are met at every point by some objection. We have been studying light for hundreds of years; we are not anywhere near satisfied with the theory yet, and we cannot very well be expected to be satisfied with the theory of Röntgen rays in one year.

Well, I think that is all I can say with regard to the subject, and I hope the other gentlemen who are to carry on the discussion will satisfy you on all these points that I have brought up and left unanswered.

Prof. Elihu Thomson:—Mr. President and gentlemen of the Institute:—I have been very much interested in the expression of opinion by Prof. Rowland as to the nature of these rays. I can certainly second his statement that we know very little about them; that is, as to their real nature, and the more facts we accumulate they seem to be carrying us, if anything, farther away. We may have a dozen different theories, and we do not yet seem to have any proof of any of them. I have not, however, given very much time to considering that side of the question; I have been working somewhat, as leisure time allowed, in finding the

conditions under which these rays were produced, and in obtaining them, if possible, in great amount.

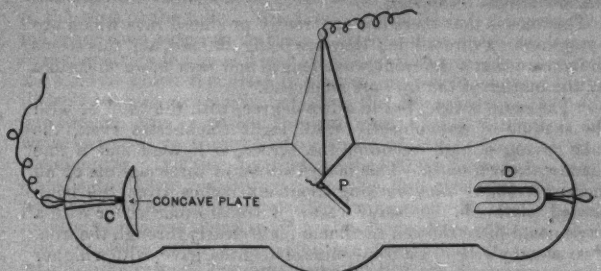
I can hardly agree with what Prof. Rowland says in regard to the rays not being produced when the cathode rays strike anything but an anode, unless this qualification be admitted—that for the time being, the thing struck becomes an anode by induction. That, of course, may be the explanation; that may be the way out; because we have many tubes which have mounted in the interior an insulated piece of metal—I had a very active tube in which an insulated piece of platinum was mounted opposite two concave pieces of aluminium which were made alternately the cathodes. This insulated piece mounted on a glass stem, without any connection on the outside, was a vigorous source of rays, and unless it became an anode by induction, we must admit that a piece of metal bombarded becomes a source. So the glass of the tube may become a source by being bombarded, and a source in all directions. The rays are transmitted through the glass; they are transmitted back from that point also laterally and in fact in all directions. I have come to regard it as a settled fact, that if the cathode rays strike a piece of any substance and particularly a dense substance like platinum, unranium or iridium, and these cathode rays are directed in a right line toward the surface, they produce Röntgen rays. If, however, they are diffused, as by crossing the focus of the cathode and then spreading out, as when the vacuum is a little too low to allow them to go on as a jet, then the rays cease to be produced. That condition is present in a tube too low in vacuum, and you can generally recover it by working the vacuum up.

As to the methods of excitation of tubes, I have found that static machines, induction coils, high frequency coils and various other apparatus for giving high potentials, all under proper conditions, are about equally satisfactory. For a static machine to be used, it must be of large capacity. In other words, it must have a large watts output relatively for such machinery, and a multiple plate machine therefore, driven with high speed, as one made with hard rubber plates driven 2,000 revolutions and say 24 plates, is a wonderful source of discharges for the tubes. It will keep the platinum hot in the tube, making a bright spot on the platinum. The spark itself between the terminals of such a machine will almost instantly set fire to combustible materials put in between the terminals. A vigorous induction coil gives, of course, uni-directional discharges, like the Holtz machine, except that they are intermittent, whereas the Holtz machine can keep a Crookes tube apparently continuously lighted; that is, the intervals between the discharges are so exceedingly small that the most continuous possible effect is obtained when the static machine is used. It has, at it were, a constantly impressed electromotive force, and the output of the machine is, as it were, a constant current under that electromotive force, but no doubt

divided into intermittent discharges at a very high rate in passing the tube. An induction coil driven with 50 to 100 breaks per second, is an admirable source of rays when the tube is proper for it, and in that case what is called a single focus tube is used. When we come to the high frequency alternating discharges we have the double focus tubes with two cathodes generally placed opposite each other and the rays from which bombard in common a piece of platinum in between, and if that platinum be made in the form of a wedge, with a somewhat acute angle, the two bombarded spots or sources from either side may be so near together as to give practically one focus for ordinary uses. In that case the tube, if exhausted properly, is a vigorous source of rays.

But the most effective method of excitation which we have yet been able to use has been one of which the public has heard nothing so far, and I mean to speak of it now. It was arranged by Mr. Hermann Lemp (whose name is familiar to many of you) in this way: He simply took a 12-inch inductorium, a coil giving a 12-inch spark ordinarily, and excited the primary with alternating currents at 125 cycles. This gives in the secondary, as in any step-up transformer, a great increase of potential, such that we may find that the spark darts five or six inches between terminals, and, of course, after it is started, there is a continuous arcing somewhat difficult to stop unless you blow it out or shut off the current, which latter is probably the easiest way. Now this high potential discharge of the secondary is, of course, an alternating current—an alternating current somewhat of the same wave form as the impressed primary wave. But if we rotate, by a little synchronous motor, a break piece which picks out one direction only of the secondary discharge and leaves that in the other direction open-circuited, then you see we have an admirable source of uni-directional discharges of great power. Of so great power indeed are they that you would not dare to put them upon your Crookes tube without modification. You must put in a high resistance, such as a water resistance—a long glass tube filled with water—to cut down the flow which would destroy almost any tube you might try. The commutating device made properly simply consists, for example, of two insulated terminals in series with the discharge, and a connecting wire revolving synchronously between them. With a spark gap between the wire and fixed terminals, of course we do not need friction or any contact. You are then chopping off the tops of the waves or are taking the very highest potential of each and every wave all in one direction, and you are also giving them a spark gap between the fixed terminals and revolving wire which is favorable to the generation of the rays. The spark gap regulates itself in a measure, because just as soon as the potential is such that it can easily jump a gap, then the commutator wire anticipates the discharge and the current leaps the spark gap. If there should

be a weaker discharge, the wire comes up nearer to the terminals before the gap is jumped, so that in this case the discharges are wonderfully uniform and you get them at the rate of 125 per second, which is a rapid rate, and excites tubes wonderfully well. With this means of excitation which I experimented with about a week ago for the first time, I was astonished at the results obtained. Unless you are very careful, you melt right through the platinum electrode. Half a second might be sufficient. In fact, we did that, but it did not hurt the tube any as a source of rays. There existed just back of the platinum plate a brace of thicker platinum which did not melt. The rays did not strike that thicker piece squarely; they only struck it on about one-half the area of the hole formed in the platinum sheet. A singular thing occurred in this case. I will draw a sketch of the tube which was used (see Fig. 1.) Here, at P, was an inclined plate of platinum sealed in from the side, supported from this side,



GLASS X RAY TUBE

FIG. 1.

and with a little rib of platinum on the back, and at this end, D, a dummy terminal of no particular use. It was simply put in there because it was thought it might be of some use—and that is sometimes a good thing to do in these tubes. Then we had the concave cathode, at C. Now the rectified current was sent between these C and P and a hole was instantly bored through the platinum P and the rays struck the little rib back of it. The rib was about one-half exposed to the hole after it was made, and half the size of the hole was opened for the passage of rays clear through—cathode rays, of course, in this case. The result of this was that in about three seconds this piece of aluminium, about an inch and a quarter or an inch and a half wide and a thirty-second thick, was red hot. It was repeatedly heated in three or four seconds by the cathode rays that had apparently passed this rib and gone through the hole. This shows the enormous vigor of the cathode rays. When this tube was used there was a white hot area all around the hole in the platinum plate.

In using the fluoroscope, or fluorescent screen in the dark, I found that a most intense sharp shadow of the bones in the hand and a shadow comparing with those obtained by photography was thrown upon the screen, and the details were beautiful—although the image was formed within only a short distance of the tube. But the astonishing thing was the flood of light obtained, and the fact that you could take the screen away and for many minutes afterwards see that shadow glowing—or rather the space around it on the screen glowing. I would like to read here in this connection some few notes made at the time I quote:

"When the vacuum was so high that but very little fluorescence existed on the sides of the tube, the rays got through four thicknesses of iron over one thirty-second thick—that is, an actual thickness is three-sixteenths—and cast strong shadows of a brass disk or plate about an inch thick. When the vacuum is lowered the screen becomes very much more luminous, but the shadow almost disappears."

This means that there are apparently produced rays which are not going in a direct line; they are being diffused, and this seems to indicate that a different wave length and rays more diffusible by the matter of the iron are produced.

"The same is true, but to a less degree, with the hand or with the shadow of any object. With eight thicknesses even, the tube having a high vacuum, the screen is lighted and a fine shadow thrown on it. This makes a total of three-eighths of an inch of wrought iron, working about six inches from the bombarded platinum. A heavy cast-iron transformer cover with a brass name plate showed the name plate clearly through the iron. Two metal pieces—cast-iron—nine-sixteenths, gave, with a higher vacuum, an enormous amount of rays, but no distinct shadows could be seen passing through these plates, owing probably to the fact that the metal may be a very strong diffuser of the rays."

Now that last statement is a very curious one. Take two heavy plates of iron about nine-sixteenths thick and slide them over each other so as to get over half the screen a double thickness, and it was very difficult to discern any difference between the part with a single plate and that with the double thickness between it and the source. If the current was cut off after the tube was excited, the luminosity of the screen disappeared. There is only one other explanation of this phenomenon that I am able to offer, and I offer it, having experimented too little to say whether it is a correct explanation. It is just possible that the screen is not illuminated by the rays coming *through* the iron, but from the diffusion of the rays from the operator's body. The only way to avoid that diffusion would be to get into something like an armor plate casing and work through a hole, so as to be able to cut off all these diffusion rays that are thrown backward.

Prof. Rowland has told you about the general characteristics

of these rays in their relation to the cathode rays, and I wish merely to call attention to one or two additional points. In working a tube, say a single focus tube, with an inclined plate of platinum, used as anode, it has always been noticed that when it did its work vigorously, there was an area of fluorescence on the glass wall extending from the plane of the platinum and covering the whole of the walls on the bombarded side. This illumination is uniform apparently all over those parts of the glass walls of the tube that are centered around the bombarded spot on the platinum, and that uniform illumination never comes unless you get Röntgen rays. They are evidently produced by the Röntgen rays passing the glass outwardly and illuminating or making the glass fluoresce. But, curiously, you can see such fluorescence in daylight or in a room fully lighted, but if you pulverize some of the same glass and make a fluorescent screen of it—experimenting with different thicknesses, you do not get more than the faintest action even when you put the screen close to the glass of the tube. What does that indicate? It seems to me there is no escape from the conclusion—that there are some rays that strike the glass which do not go through—that they are the kind to which the glass is opaque, or nearly so, and very little of them get through; whereas, the Röntgen rays that do get though, having passed through glass, (being filtered as it were) can now go through glass again or other things of the same nature. It is the quantitative effect of the fluorescence that leads us to such a conclusion. We also find that cathode rays produce fluorescence wherever they strike, and this leads us to inquire whether in doing so they always produce Röntgen rays first, or these other rays which are lower than Röntgen, or whether the cathode ray does alone produce the fluorescence. I think that this question would be one of the most difficult to experiment upon or to decide. That there are varieties of Röntgen rays that differ in some way (whether in wave length or what not it is hard to say) is undoubtedly a fact beyond all question. They cannot be divided, in my opinion, into X — 1, X — 2, X — 3, or to put it differently, into flesh, wood and metal rays; but they probably represent, if they represent anything, a sort of gamut—a variation upward and downward in the scale. In working the tube Fig. 1 and lowering the vacuum somewhat, using an iron plate three-eighths thick and the screen in front, you get very little effect at first or not until the tube works up, and it works up in half a minute or a minute. You then see the screen getting brighter and brighter all the time until the screen is illuminated quite strongly. This evidently indicates the production at the last of apparently a different sort of rays which penetrate the metals even when of considerable thickness. But if you put your hand or any thin metal object back of the heavy plate when the rays pass freely, you get a very faint shadow indeed, showing that these rays which get

through the metal, get through the hand, if anything, more easily. They are the rays which will get freely through a man's body, perhaps through great depths of it. It probably requires these very rays to pass through any great thicknesses and this may account for the fact that it is very difficult to get any strong impression of the vertebral column, for example, in the body, because such of the rays as are able to pass through great thicknesses of tissue have also a high penetrating quality, even for bone.

Another fact which I noticed in experimenting with this tube (Fig. 1) is, that adding sheet after sheet of iron, each a little over $\frac{1}{32}''$ thick, beginning at 1, then 2, 3, 4, when I got to about four sheets my object shadow was black and very clear and distinct. When I got to five or six sheets that shadow was beginning to be faint and blurred, and at eight sheets could just about be distinguished with care. What was very strong and vigorous at four sheets had apparently almost disappeared at eight sheets. Now unless there is some other explanation, this would indicate that the rays can, as it were, go a certain distance and are stopped or diffused, or that certain rays are filtered out that with the lower thicknesses, gave the shadow; while those which were able to penetrate the greatest thicknesses kept right on and penetrated object and metal all together. But then a caution comes in here. It is possible that the back diffusion of rays from the operator not working back of a heavy metal shield is illuminating the screen by diffusion from the surrounding objects, and we may have an effect something like the opposing lights in a Bunsen photometer which, in a certain definite amount neutralize each other on the screen. Such is a possible explanation of the effects. There is needed much further experimenting.

It is curious to notice with the tubes having comparatively low vacua that no effect of production of rays occurs with the ordinary passage of a silent discharge, as by the Holtz machine, but the tubes often become good sources of rays if you put a condenser on the terminals and use a spark gap. A tube which is absolutely of no use for a steady discharge from the terminals may be made oftentimes very active by this simple expedient.

We often come across very curious things in this work with different forms of tubes under different conditions, and I think there is hardly a more fascinating field than working with these vacuum tube arrangements. There are no two alike. You can hardly produce the same exact effects twice. I may mention a curious thing which I noticed the other day. We had a spherical bulb with a wedge of platinum, the cathodes opposite each other. We made one of these actual cathodes, and the platinum the anode, with commutated current excitation. The vacuum in the tube was a little low, and one would suppose that the side of the platinum nearest the real cathode would get hot, but I was astonished to find that there was a bright spot on the other side opposite the idle cathode and none on the side opposite the real

cathode. This latter side was not even hot, but the apparently idle cup on the other side of the tube had sent out something or other which produced a red hot spot opposite to it and gave rays. That is only one of the curious things we find, which among the many other serve to mix us up and carry us perhaps farther away from the real thing that we all are looking for.

In regard to the diffusion of the rays, I have to say a word or two. Some time ago I tried to start some experiments in carrying on the work of investigating diffusion of Röntgen a little more fully with an object in view. My idea is represented in about this way. (Illustrating): This represents a vertical metal screen M. Fig. 2. I placed a Crookes tube here at T. I put a block of paraffin here at P that I can turn about. I put a heavy metal screen at the back at N. I placed a fluorescent screen here at F, facing in the direction of the arrow and shield-

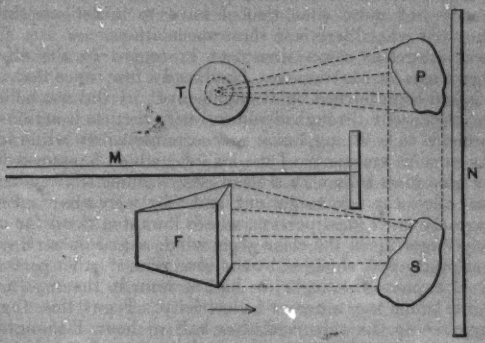


FIG. 2.

ing it in every possible way by metal. If then I get from this paraffin P, diffusion (which I know I can get from back, sides or anywhere) it simply behaves like a lump of opal substance, and I have the screen F in position such that no ray could reach it from P; then if I put another block of paraffin here at S and I get rays from it back to F I have what I may call secondary diffusion. If I then get the same effect from S as from P it indicates that the rays from P are Röntgen rays and answers the very point that Prof. Rowland raised in regard to diffusion. They would be Röntgen rays, and again doing the same thing that they did at first at P received at S and sent out again by the secondary diffusion. I tried some experiments in this direction, but I found my screens were not thick enough. I thought I got some effect, but the work will have to be done over again.

There was another point in this connection which interested me

very much and will require a good deal of time in experimenting. I was going to put a substance like paraffin here at P and say wood there at S and then interchange them. Now, if we do this, and do it enough times and with many different substances, we may be able to—especially if we could measure quantitatively the relation or effect—to discover that there is a spectrum or color value for each substance. If this at P, for example, absorbs certain wave lengths and sends out others, and this substance at S being the same substance, passes them on, and then is replaced by another substance which has a different Röntgen ray color, absorbing more of the diffused rays, the screen would be darker. Interchanging the positions and changing the substances to any length would enable us at last to get at the spectrum, if there was any such thing, and at the same time to say that paraffin had a certain ray color, so to speak, and so on for other substances. This is in the future. I simply mention it now, wishing that somebody had more time than I have to investigate this field and find out what there is in these speculations.

I was interested some time ago in regard to the effect of Röntgen rays on the tissues. I had read a few times that certain people had been burned by Röntgen rays. I did not believe it. These rays went through tissue so easily that their action could not amount to anything, but it was certainly worth while investigating so as to know. So I used a tube, which happened to be a heavy blue glass tube with a clear glass window. The blue glass did not allow the rays to get out, and they were absorbed except through the clear glass portion, where I wanted them for use. I put my finger up to the clear glass window and kept the other fingers pretty well shielded by the blue glass of other parts of the tube. I exposed the finger for half an hour to the rays, a Holtz machine being the source of electricity. I put the finger up pretty close to the tube, and after half an hour I thought that perhaps it was not long enough; perhaps it was not half enough. But if there were to be any effect it would be equivalent to a few hours distance, and as I got tired I went no farther. I shut down the tube and went away. Five, six, seven, eight days passed and nothing happened, and I felt that people had been mistaken about the effect of the rays. But on the ninth day the finger began to redden; on the twelfth day there was a blister, and a very sore blister. On the thirteenth or fourteenth day after exposure, the blister had included all the skin down to the part not exposed and had gone around the finger almost to the other side. The whole of the epidermis came away and left an ulcer without any possibility of recovering its own epidermis except from the edges, and I had to go through that painful process of having a raw sore there and the epidermis growing in from the side and gradually closing up. Only three days ago was the sore actually closed, and the skin is yet very tender, and nature does not appear to have found out how to make a good skin over that

finger. The skin still comes off in flakes and is very disagreeable and very tender, and there is a burning, smarting sensation every now and then. But I am satisfied it is coming out all right. I showed the finger, when at its worst, to my family physician. He looked at it and said: "Do you think you are going to lose that finger?" I said: "No; I don't think it is as bad as that;" but I must say that for a time it was a very angry looking affair. Some one has said that this is an electrostatic effect; the rays could not do that; you have got an electric burn, and they are notably difficult to heal. This has taken six and a half weeks to-day. But that view, I think, is negatived by a case reported, where a girl at Oberlin College was made the subject for examination through the chest, and her clothing was not removed, and yet she suffered from a very extensive, large ulcer and had to go to the hospital and stay there for treatment for quite a while. The doctor who had charge of the case told me of it and said it was a very angry looking sore indeed, and the astonishing thing was that it was accomplished through her clothing. Now if it had been electrostatic or ozonic, or in accordance with other theories that have been put forward to account for it, I don't think it would have occurred through the clothing. It must have been the radiation; but whether it is the radiation of Röntgen or some other kind of radiation is another question, and that is still open. It seems to me that what is likely to produce it is the lower rays; not the ones that go through metal but those of the lowest order, and this seems to be indicated by the fact that it stopped on the edges of the finger and did not reach the other surface. If it had been the rays that go through, I think it would have been the whole finger that would have been affected. It possibly has been a selection out of the lower rays, and if in examinations we could screen those off, perhaps we would have no trouble in this way.

I must tell you a rather amusing incident in this connection which is somewhat of a joke on myself. We had a mouse caught about a week ago. I thought, now, here is a good subject; perhaps I can take the hair off him. So I put him in a very small wooden box with sides about an eighth of an inch thick and exposed him for an hour to a very intense source of rays through the wood of the box. I put the mouse away in another box—I did not want him to run around in the first and get out of the sphere of influence—I put him in another box and had a little glazed cover so he could get air and I could see him inside. I put him on a pretty high shelf in the hall at the house. One night I was writing at my desk at about eleven o'clock when I heard a noise that made me think the clock was getting ready to strike. As soon as I got ready, I went out and there was the cat on the shelf—and the mouse gone. If we ever see a hairless mouse around the house after this we will know what was the cause of it.

I do not know that there is very much else in this connection

that I can say. I did not intend to speak about theory, and I will give place to others. If anything occurs to me later, and there is an opportunity, I may add something.

DR. M. I. PUPIN:—After a discussion of this subject by two such men as Professor Rowland and Professor Thomson, it is difficult to add anything. They have certainly said a great deal about it. They both confessed their ignorance of the subject, and five minutes ago I wondered how much more they would have said if they had known something about it. A few more words only can be added concerning the experimental side and still fewer concerning the theory.

When the discovery was first announced here, I, of course, was just as anxious as everybody else was to make myself familiar with the new radiance. But I had neither an induction coil of an effective size nor Crookes tubes, and so I used the high-frequency coil and vacuum tubes without internal electrodes; the results obtained were good enough for that time and I was perfectly happy. Professor Lodge confirmed my work regarding the possibility of obtaining Röntgen rays by tubes without internal electrodes. The experience which I obtained with the high-frequency coil was not a pleasant one, because it used the tubes up too rapidly. In fact, after exhausting my stock of electrodeless tubes I obtained some genuine Crookes, and managed to break most of them with the high-frequency coil. Then I thought I would change my generator for the sake of saving tubes; so I borrowed a six-plate Holtz machine and some Crookes tubes from Professor Doremus of the College of the City of New York and obtained rather good results in this way. In fact at that time I thought that the Holtz machine was very much better than anything else. I had not yet tried a good induction coil. But even now I agree perfectly with Professor Thomson, that the influence machine of large output would be an ideal machine, only it is too expensive a piece of apparatus and perhaps somewhat too bulky. An induction coil proved, so far as my experience goes, more satisfactory than anything else—an induction coil of large output—so that one can always have reserve power and can call upon it when he may choose. Now it seems to me that in the use of the induction coil there is a great deal of difference of opinion as to the modus operandi. I have used as many as 120 breaks per second, and others have used 200 breaks per second; and I have used also a rotary current interrupter, thinking—and I believe my opinion was correct—that a rotary current interrupter would avoid sparking, and give a much cleaner break than a vibrating current interrupter. Yet I have seen most creditable work done by ordinary vibrating interrupters, where the number of interruptions was not more than 10 to 15 per second, or even less. I am not very sure that it is quite clear why a small number of breaks can evidently produce just as good effects as a large

number of breaks, both employing the same spark-length. This seems to me to be the most important practical point to investigate, because it is by the solution of the problem of producing the X-rays in the most efficient way that we can expect to solve a great many other pending problems, like, for instance, the attempt which Professor Thomson described here for increasing the wave-length by successive diffusions. In this case the chief difficulty would be that we would have to use substances like paraffin, wood and so forth for the diffusion effects, because as I demonstrated last spring[1], insulators, as a rule, especially wood and paraffin and so on, are the best diffusers, but at the same time the best transmitters of the X-rays, so that only a small proportion of the X-rays are diffused by the substance, and if you use successive diffusions there will be after three or four diffusions very little left to experiment with. Hence the necessity of producing the X-rays in as large quantities as possible. Now there

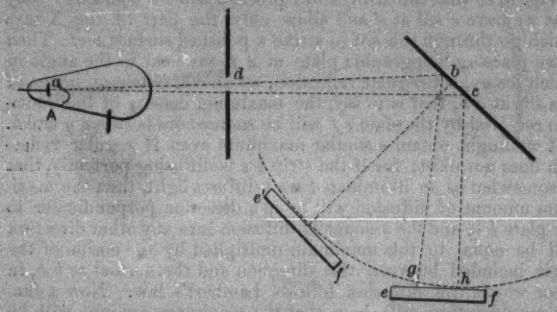


FIG. 3.

is no doubt that a certain quantity of energy per second must be sent through the Crookes tube in order to produce the best effect, and the question arises whether we are to follow what may be called the homeopathic or the allopathic way of sending that energy through the tube. That is to say, you can send 5 watts in doses of one one-hundredth part by 100 breaks per second, or we can send these 5 watts in doses of one part at a time by having only 5 breaks, and then the question arises, in which case will we get the best effect? Preliminary experiments that I have made so far (but I have not done much lately on account of illness) seem to show that up to a certain limit the best way is to use large quantities of energy at large intervals and not small quantities repeated in rapid succession.

Now as to diffusion and specular reflection of the X-rays.

1. *Science*, New York, April 2, 1896. *Science*, April 10, 1896.

Last spring I was trying to produce a reflection of the X-rays at about the same time other experimenters were doing the same thing, both in this country and abroad. Some of these men obtained what they supposed to be regular reflection. The method of procedure, as you all know, was to place a polished reflecting plate at a certain angle to the X-rays, and then place perpendicularly to the direction in which we would expect the X-ray beam to be reflected, a photographic plate, and if an image is obtained, that would be some sort of a proof that the X-rays were reflected. At that time it struck me that this does not constitute a valid proof that the X-rays are reflected, since diffusion would produce a similar effect. In all experiments of this kind, the effect of regular reflection will be very much marked by the effect of diffuse reflection, and now permit me to say a word or two on this point and also to suggest a way in which this question may perhaps be decided. Suppose that we have a tube at *A*, Fig. 3, so that the X-rays will proceed along *a b* and *a c*. Say that we have a slit at *d* and allow only the part of the X-rays which go through this slit to strike a polished surface *b c*. Then if we place a photographic plate at *e f*, say near to the angle in which we expect regular reflection, we should obtain an image of this slit at *g h*, that is to say, the maximum density of the radiation received by the plate *e f* will be somewhere between *g* and *h*. But we might obtain a similar maximum even if regular reflection does not exist; for if the strip *b c* is diffusing perfectly, that is, somewhat as an illuminated wall diffuses light, then the maximum amount of diffusion will be in a direction perpendicular to the plate *b c*, and the amount of diffusion into any other direction will be equal to this maximum multiplied by the cosine of the angle included between this direction and the normal to *b c*; in other words, the diffusion follows Lambert's law. Now a simple consideration will show that the maximum amount will be received by a strip along *g h* where we get a maximum by regular reflection; so that the maximum at *g h* does not by any means indicate that there is regular reflection; because, as I said, diffusion will also give a maximum *there*. Now, suppose that both diffusion and regular reflection exist; then the two effects would be superposed there, and we would not know how much is due to regular reflection and how much is due to diffusion. If, however, we use at *b c* instead of a plane reflecting plate, a spherical mirror *b, c, d*, Fig. 4, and allow the X-rays *a b*, *a d* to fall on this spherical plate, then the part which is diffused will be brought to a focus at the centre *e* of this mirror, because a radiation by diffusion is always maximum normally to the elements of the surface, that is if it is diffusion like the ordinary diffusion, and that it seems to be according to my experiments; whereas the rays that are regularly reflected would be brought to a focus at a different place, somewhere in the vicinity of the point *f*. In that way we could separate the two, and also by measuring the time

of exposure at the points e and f get approximately the comparative strength of the two reflections.

The subject is sufficiently interesting to deserve a careful experimentation of this kind, although, of course, it would be very difficult to produce desirable effects, because we use a polished mirror like a speculum-metal mirror and allow only a small amount of X-rays to fall on it, reflected radiation is extremely small and the time of exposure would have to be excessively long. Still I think that the effects can be produced, and I intend, as soon as my health will permit, to make experiments of this kind.

Now as to the theory. With your kind permission, I venture to add a few words to what Professor Rowland has already said, and wish to call particular attention to some of Professor Helmholtz's theories. This is the more desirable as one of these theories has been criticised lately rather

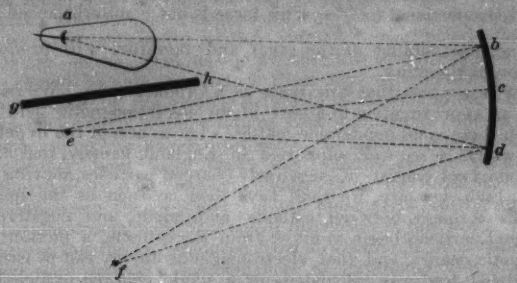


FIG. 4.

harshly by Mr. Oliver Heaviside, owing undoubtedly to a misunderstanding on his part. Among the various theories suggested last spring to account for this X-ray radiation was one which Professor Rowland did not mention, but which I shall briefly describe, because it resembles somewhat closely the theory proposed by Professor Stokes: namely, the theory that in the X-ray radiation we may probably have a sort of circulating motion of the ether. You know that according to Maxwell's electro-dynamics, the stresses in the electromagnetic field are distributed in such a way as to produce a pondermotive force acting on the ether, and this pondermotive force is, as a rule, counterbalanced by the incompressibility of the ether; so that, as a rule, there is no motion of the ether under the influence of this force. For instance, a rubber ball when it is compressed by a uniformly distributed normal surface pressure, counterbalances this pressure by its elastic reaction and there is no motion of the rubber ball as a whole. Helmholtz was the first to propose

the question whether it is possible that the ether should move bodily, owing to the action of pondermotive forces not balanced by its incompressibility. It is not necessary to go into any detailed discussion of what would happen in the ordinary magnetic field. I will only mention the case which Helmholtz discussed, namely, the case where there is no ponderable matter in the ether. Suppose that we have a space containing no ponderable matter; the question arises then, can there be such a distribution of stresses, or in other words, of electric and magnetic forces, as to produce a motion of the ether there. Helmholtz gave an answer to this question and proposed—this was in 1893—to go into an additional discussion of the same problem, but unfortunately he died in 1894. This is the answer which he gave: Suppose that there is motion of electromagnetic energy; then say that the velocity of this motion in a given direction is p, then the pondermotive force acting upon the ether in that direction is proportional to $\frac{dp}{dt}$. This force is not counterbalanced by the incompressibility of the ether and therefore will produce motion of the ether.

Let us suppose that this space is a Crookes tube. We have then an approximation to a part of an electromagnetic field in which there is no ponderable matter. Let us accept the hypothesis which is so well supported in England, namely, that the cathode rays consist of negatively charged particles, moving at very high velocity, we shall have then as soon as the spark breaks through between cathode and anode, and negatively charged particles begin to move from the cathode, a motion of magnetic energy, and that electromagnetic energy can be easily calculated, and was calculated for a single charged particle by Professor J. J. Thomson in the first chapter of his book on "Recent Researches in Electricity and Magnetism." The motion of that electromagnetic energy is in the same direction in which the particles move. The lines of force of the magnetic field produced by a motion of a charged particle are meridian circles to which this line of motion is an axis. Lines of electric force are everywhere perpendicular to these circles. Energy moves along lines which are perpendicular to the electric and magnetic lines of force. We have then, when a negatively charged particle begins to move from the cathode, a beginning of motion of energy—the velocity changes from zero to a certain very large quantity. Therefore we ought to get, as soon as the discharge commences, a pondermotive force which tends to move the ether in the direction of the cathode rays, and this force will be the more intense the more disruptive the discharge is. When the negatively charged particle strikes an obstacle, say the glass wall of the mercury tube or the anticathode, we should have then the energy flux changing from a very large value to a certain very small value. We get then again, a force again tending to move

the ether in the direction directly opposite to the cathode rays. According to this view the cathode and the surface struck by the cathode rays become the seat of points from which radiate forces tending to produce a bodily motion of ether. Whether they really produce an actual motion of the ether or not, and what the nature of this motion is—these are questions which can be decided by the coördination of experimental facts derived from the study of the X-rays. This possible view of the X-ray phenomena was suggested by me in an article which I published in *Science* of April 10th, 1896, and I still consider it worthy of some consideration, although I must admit frankly that in our present state of knowledge of the physics of X-rays, very little weight only can be attached to any theory.

There is another view which I wish to discuss briefly, and that is the view offered by Helmholtz's electromagnetic dispersion theory. You have heard it mentioned that the X-rays are in all probability ordinary light of very short wave-length. Then, according to Helmholtz's dispersion theory, these wave-lengths may be so short as to suffer neither refraction nor polarization by ordinary matter. Let us see whether this theory explains how we should be able to produce these rays of excessively short wave-length by the electric action in a vacuum tube. Every dispersion theory which explains how a substance will act with respect to certain waves, must also at the same time be capable of explaining how these short wave-lengths can be produced. Otherwise it does not completely fulfil its function. The Helmholtzian theory differs, in my opinion, radically from all other electromagnetic theories of dispersion in this way: Some of these theories are produced from no definite physical basis, but start with tentative mathematical assumptions. The physical basis of the others may be obtained somewhat as follows: Suppose that we have an electric system consisting of say seven parts, each part containing self-induction, capacity and ohmic resistance. This system of electric conductors is a vibratory system, and if distributed by an electric impulse it would become the seat of electric vibrations, not vibrations of a single period, but of seven different periods. That is to say, in each of the seven conductors we should have an electric oscillation composed of seven singly periodic vibrations. The amplitude of each particular frequency in any conductor depending, of course, on the constants of that conductor. A system like that when disturbed, will send forth electrical waves of seven different wave-lengths, and if we could use suitable means like prisms, we should get if the waves are sufficiently short an electric spectrum, because of course, for each of the seven different waves the prisms will have a different index of refraction. Now suppose that we diminish the size of this electrical system until we get the molecular dimension, we would then have what may be considered a molecule consisting of the component atoms, each atom, as it

were, having all the electrical properties of an electric conductor, namely, self-induction, resistance and capacity. Each molecule would then have a multiple period of vibration; and therefore capable of sending so many different wave-lengths, which sent through a prism, would give a spectrum of the molecule.

Now several of the more important dispersion theories to-day, are based on physical conceptions of this kind—that the molecules consist of component parts, and each component atom is in a certain sense an electric resonator, having the properties of self-induction, capacity and resistance. This assumption forms the weak point of these theories. Now the Helmholtzian theory is radically different in that it makes no assumptions of this improbable kind. The physical basis of the Helmholtzian theory is suggested by the experimental fact which is the foundation of modern electro-chemistry, the fact namely, that to every velocity of an atom, there is a definite quantity of electricity attached, and that therefore in a molecule we have in each component atom a perfectly definite quantity of electricity. Then, according to Helmholtz's theory, if there is electric force in the ether, say an electric wave, that electric wave will act upon the molecule by acting upon the electrical charges which are attached to the valcencies of each component atom. Such a molecule when disturbed will become the seat of electric vibrations, not vibrations such as take place in an electric circuit, but vibrations which we get by causing a charged body to oscillate. This kind of electric vibration in the molecule is determined not only by the electric relations between the component atoms, but also by the mass of the atom, and also by the mechanical force acting between the atoms in the molecule. Helmholtz's theory discusses these vibrations in a molecule consisting of two atoms, but it is not at all difficult to see what would take place in a more complex molecule. A molecule consisting of say a thousand atoms would have a thousand different periods of oscillation. In fact each atom in the molecule vibrates with a complex harmonic vibration, consisting of the superposition of a thousand simple harmonic vibrations of different periods. Some of them may be very high and some very low, comparatively speaking; and whenever an electric wave of a periodicity corresponding to one of these periodicities strikes the molecule, there is resonance; that is, the wave is very much absorbed. The index of refraction of a wave passing through a substance having resonating molecules, will be quite different than the index of refraction for waves which awake no resonance in the molecule, since both the index of refraction and the coefficient of absorption for a given wave depend on the resonance relation between the wave and the molecule.

This short account of the dispersion theory suffices to prepare us for an explanation of the manner in which these very high frequencies and the accompanying excessively short waves which

would correspond to the Röntgen rays could be generated by the discharge in a vacuum tube. Consider a vibratory system consisting of a thousand different but interconnected parts. It has a thousand different periods of vibration, and each component part will vibrate with the same complex harmonic vibration consisting of a thousand simple harmonic components. The relative values of the amplitudes of these simple harmonic components depends altogether on the manner of excitation. Take a bell, for instance. If you strike a bell with a small hammer and give it a very sharp tap you will bring out the high shrill notes in the bell. That is the higher vibrations will be brought out more prominently than the lower ones. In the same way if a molecule is excited by striking the molecules against each other, as occurs during the molecular motion which corresponds to sensible heat, then the higher temperature, the quicker, the sharper will be the tap, the stronger will be the higher vibrations. This is the generally accepted explanation for the shifting of the spectrum toward the blue end as the temperature increases. In the Crookes tube we in all probability have the best method of exciting the very highest vibrations; because here the negatively charged particles which are torn off from the cathode are moving with enormous velocity, and wherever they strike an obstacle, say the glass of the tube or the platinum plate of the anode, there they produce an impulse of the very highest degree of sharpness, very much sharper than any existing temperature has yet been able to produce. The amplitudes of the very highest vibrations in the molecule are made prominent, and they manifest themselves as the X-rays. But if the X-rays are really transverse vibrations, then we should in all probability along with them have other vibrations of lower period also but only weaker, just as in the ordinary excitation by means of the electric arc or by a Bunsen burner the amplitudes of the lower vibrations are brought out prominently, but the higher vibrations are also present but only much weaker.

Helmholtz's theory was criticised lately by Mr. Oliver Heaviside, but unjustly, owing undoubtedly to a misunderstanding on Mr. Heaviside's part. Mr. Heaviside applies a peculiar test to this theory; the test, namely that the system of mechanically vibrating ions should satisfy certain conditions which exist in an electrical system composed of coils and condensers, such as I have described above. If we examine a little more closely we will see that the Helmholtzian theory, although it is not called upon to satisfy conditions of this kind, does actually satisfy them, provided, however, that a single mistake, which was corrected a year ago by Dr. Reiff, is corrected. This mistake does not affect the main results of the Helmholtzian theory, and there was no necessity to worry so much over it as Mr. Heaviside did, especially when there is danger that his criticism might produce the impression that the Helmholtzian theory, which was brought out

very prominently in connection with the X-ray phenomena, might have some weak and inconsistent point in it, and therefore might be misleading. It is not so, but on the contrary it is one of the most beautiful and most suggestive theories in this connection.

Dr. A. E. Kennelly:—Mr. President and Gentlemen:—I have been so much fascinated with the delightful remarks we have heard this evening from some of the earlier speakers, that I only regret that in a weak moment I consented to join in this discussion, and that I have so little to add to it.

Just one word about the theory of the subject. Whatever may be the nature of the Röntgen ray, it must be either a bodily movement of matter, or a movement of a disturbance in matter, or a bodily movement of ether; or, finally, a movement of a disturbance in ether. As regards the first supposition, that X-rays are streams of matter in motion, or of projected molecules, there is an experiment which seems to negative it. A Crookes radiometer vane does not, as far as I have been able to discover, recede from an excited Röntgen ray tube. One would suppose that it would be powerfully repelled, if X-rays were bombarding streams of particles.

As regards the three remaining hypotheses, it is to be hoped that future researches will show that X-rays are ultra-ultra-violet light rays. Not only would this be the simplest conception that we can at present frame, since it would only call for further extension of the spectrum, but it would also keep X-rays within the limits of electromagnetic waves and in the immediate province of electricians.

In reference to the applications of X-rays, I have made a few experiments, in conjunction with Professor Houston, upon the perception of X-rays by the blind. These experiments seem to show that where the mechanism of the retina has been destroyed, leaving the optic nerve in a useless or atrophic condition, no X-rays are perceived. When the mechanism of the eye is intact, but the optic nerve is deranged or paralyzed, some visual conception may be obtained by the stimulus of X-rays. When the optic nerve and retina are both intact but the cornea is deranged, the fluorescent effect of X-rays upon a calcium tungstate screen held before the eyes, excites the visual sensation in the ordinary manner to a large degree that depends upon the corneal opacity. It would seem, however, although it is not certain, that the corneal opacity may itself feebly serve as a fluorescent screen, and that X-rays filtered through wood or pasteboard falling on some eyes that are corneally blind produce a faint visual sensation of diffused light.

Mr. Max Osterberg:—I have only a few words to add, and those words are practically in connection with remarks made by Prof. Thomson and afterwards discussed by Dr. Pupin. They are in regard to the amount of energy necessary, and furthermore in

connection with the kind of vibrator used on the induction coil. We might add just one more condition and then the subject will be a little plainer. Let us differentiate between an investigation which we want to perform by means of taking a picture, and one where we simply wish to make an investigation with a fluoroscope. Of course, I, like the others, have generally used my bones, because they were in most cases handier than coins. I always had them with me. The question of energy, to my mind, is simply this: In the first place we want the energy to be in the primary circuit of our induction coil. In the second place we want this energy in the secondary of the induction coil. In order to get it into the primary, it is necessary that we have a large induction coil; that we should charge the induction coil for quite some time. In other words, the make should be rather long, while the break should be very disruptive. We cannot possibly produce a very long, or comparatively long make with a very short break on an ordinary vibrator; and consequently we are forced to use a rotary circuit-breaker where we can make the length of charge just as great as we choose. So far as the breaking is concerned, it appears that the most essential point is that this break should be disruptive. In a fluoroscope investigation this would hurt the eye very badly, because there would be constant flickering. I have tried several times to close the circuit of the primary for some time and then break very suddenly. I found at those times that the fluoroscope would fluoresce considerably—in fact, a great deal more than it would if I kept the rotary vibrator or the ordinary vibrator on the circuit. But, of course, if you want to take a picture, and if it is true that we only get the real X-ray effect at the moment of break, then it is practically a question of length of time of exposure. If the ordinary vibrator gives us a considerable effect, we might as well have the ordinary vibrator and expose a greater length of time. But if we can put more energy into the primary circuit with a rotary vibrator, we might better do that.

As to the energy in the secondary, it seems to me that it should be properly distributed. There are three places through which the energy in the secondary of the induction coil is illustrated: First, in the secondary of the coil, then in the leading wires, and finally in the tube. It is found that unless the tube works at its very best condition for the production of these rays that there is considerable brush discharge along the wires. This cannot be regulated very well because we do not know how to regulate the tube; but it can be regulated by changing the leading wires. I think Dr. Pupin was the first one to suggest that, and I believe he must have been arguing in the same direction when he made the suggestion, and that is to introduce an air-gap in the leading wires from the secondary to the tubes. This air-gap then will, of course, be one of the parts of the circuit, and that can be varied from a very small fraction of an inch to

quite a considerable length. In that case the brush discharge is practically nothing, and the tube can be constantly kept at a certain constant fluorescence of activity.

The next question is that of the alternating current *versus* the direct. I meant to say something on that subject, but Prof. Thomson has somewhat cut me short, because he outlined or rather explained the new method which had been suggested by Mr. Lemp and employed by him; that is to use a direct current or uni-directional current in the tubes. Of course, ordinarily this has not been done, but I think that up to the present, the alternating current would be very much inferior to the direct, for the simple reason that the anode and cathode being constantly changing, there will be a constant disintegration of the platinum as soon as it becomes the cathode, and this will tend to blacken the bulb, and therefore spoil the tube rather quickly. The direct current, furthermore, will be constantly acting in the same direction, while if the alternating current acts twice, the frequency would practically correspond only to one-half the number of makes and breaks on the direct circuit, and in that way the time of charging the primary coil would be doubled by using the direct current, or it would only be half the amount by using the alternating current.

Prof. Rowland:—I made a few notes with regard to what has been said, but they are made in such a way that I do not believe that I can interpret them myself, especially as the hour seems to be getting rather late. One or two remarks, however, I would like to make. When Prof. Thomson said that he got such a large amount of rays from an insulated piece of platinum by letting the cathode rays fall upon it, he made a sketch (Fig. 1). With the exception of this end, which was flat, that is the kind of thing that I used. Now, there was absolutely no effect when *this* was made an anode and *this* a cathode, so that all the cathode rays were striking on the platinum. I have the photograph; I got no effect whatever. Now, if Prof. Thomson got an effect in *this* case and I did not get an effect in *that* case, I have got a case, at least, where none of these rays were produced by the falling of the cathode rays upon the object. It doesn't make any difference how many other persons have something in which they do get an effect. If I did not get an effect, that is one case, understand. That is the case where the cathode ray fell on an object and I got no Röntgen ray. If other people got them in other ways, why, there is something else coming in. I don't know what it is.

Prof. Thomson:—I should like to say just there, professor, if you would allow me, that I used exactly that arrangement first, and got rays with the concave cathode. The anode at this end and the interposed plate of platinum between, with that wire extending outward, is the standard form of Crookes tube—the first tube, in fact, that I used. I got not only sharp effects but rays.

The Chairman:—Was the platinum red?

PROF. THOMSON:—The platinum was red—yes, of course, and it was a vigorous source of rays. I got rays with the same tube that Professor Rowland does not get them.

PROF. ROWLAND:—Well, that has nothing to do with the point. The point that I raise is this, that there was certainly no doubt that I did not get any, and the cathode rays were falling from the object. That is the thing. Now, one thing that I wish to remark is that most people draw a tube like that. They don't say where the wires go. Mine generally went out, so that they were very far away from this object. By curving wires around in different ways I can get an inductive action. I don't doubt that I could fix up a tube so that I could get lots of rays out of any part. However, the time is passing, and I will just say one word with regard to the point Prof. Thomson raised with regard to the fluorescence over the surface of the glass. He thought something was stopped by the glass. I must say that Lenard, when he first experimented upon this subject—and I regard his experiments as quite as valuable as Röntgen's, probably—, he got several kinds of rays coming out through an aluminium window. He got rays which were deflected by the magnet, as well as others. He had not separated them, however. When the Lenard paper came to the laboratory I remarked to my students: "That is the best discovery that has been made in many a day." I immediately set somebody to work experimenting. He tried to get some results and would probably have discovered the Röntgen rays at that time if it had not been that the University of Chicago called him off, and Johns Hopkins University was very poor and could not call him back, and he had to stop in the midst of his work. They always say in Baltimore that no man in that city should die without leaving something to Johns Hopkins. Now, Dr. Pupin mentioned a means of showing whether the rays were reflected—a little reflector in which he had them brought to a focus, as I recollect it. I have read an account in which an experimenter did find the rays were brought to a focus, showing, provisionally at least, that there was some regular reflection. But these experiments should all be repeated many times before one actually believes them. We don't always believe what we read.

Now, as to Helmholtz's theory of the motion of the ether and so on—well, as I said before, what is the motion of the ether? What is motion of the whole ether? You cannot move the ether in the whole universe all at once, and if you do not move the ether in the whole universe all at once but only move a part, then it is a wave, so it amounts to the theory that I gave—an impulse, such as Stokes had. Now, an impulse such as Stokes had does not go in a straight line—it goes around corners—and it does not go in a straight line unless there are lots of waves coming out. We can readily prove that an ordinary molecule, vibrating to ordinary light, must give out a hundred thousand

waves without much diminution of amplitude, or else you cannot have the sharp lines in the spectrum that we do. The molecule must vibrate a long time—longer than any bell that we can make. We cannot find a bell that will give out a hundred thousand vibrations without much diminution. For etherial waves something must vibrate to produce them. What it is I don't know that there is any necessity for discussing, because you can discuss it forever and never get any nearer to it. Something vibrates. Now, the thing that vibrates we don't know. We don't know whether it is electricity or whether it is mechanical motion. We know nothing about it. I have often said to my students, when I showed them the spectrum of some substance like uranium, in which we were taking photographs which would be perhaps ten feet long—so fine in grain that you could not put the point of a pencil on it without finding a line. There were thousands of lines. I said to them: "A molecule of matter is more complicated a great deal than a piano. Counting the overtones and everything, you would not probably get up anywhere near the number of tones you get out of a single molecule of uranium. Therefore it rather looks as if the uranium molecule was very complicated." Of course, all those spectrum lines do not indicate fundamental tones—many are harmonics. Still it is rather a complicated thing to get a spectrum in which there are many thousands of lines. So when I come to think what a molecule is and try to get up some theory of it, I quite agree with Dr. Pupin that we don't know anything about it.

Prof. W. M. Stine:—(*Communicated.*)—In the early stages of these investigations, we were compelled to work by conceptions of the nature and conditions for the production of Röntgen rays, based largely upon *a priori* reasoning, and the contradictory views held to explain the varied phenomena of low vacuum tubes under electrical excitation. The battle of opinions which has waged almost ceaselessly since then, has left the question very nearly in the same conditions it was received from Röntgen and his immediate predecessors. That one, is both ignorant and careless who affirms that no progress has been made, no additions added to Röntgen's observations; but there is no greater definiteness of views concerning the nature of these newly discovered rays. The only gain here has accrued from the process of exclusion; though we do not know what the ray is, we have learned that it is not to be classed with a few weird light phenomena. The battle of the physicists has been a curious struggle; but, as usual in the history of science, prejudice, prepossession and conservatism made a vigorous showing, and have scarcely yet realized their defeat. Again, as has often been shown, many scientists had recourse to evolution from their inner consciousness, fled to man-built conceptions of how the universe was con-

structed, followed the leadership, in spirit, of Aristotle, instead of emulating Faraday, in a direct appeal to nature. Never has the intellectual world been presented with a clearer evidence of the futility of *theoretical* systems of natural philosophy. It has been forgotten that Maxwell succeeded Faraday, and that his self-confessed merit was that he was enabled to give exact quantitative expressions for those physical facts and relations which owed their discovery to that splendid genius which had so successfully questioned nature.

It is in the spirit of this direct appeal to nature that the attempt is here made to present results of experiment and observation, and they are offered for the reason that they have received such uniform confirmation. They can probably be best presented by studying the tube historically. The results here stated were obtained from both foreign and domestic tubes, including a great variety, both in size and design.

It will be assumed that the tubes are in all cases excited from an induction coil, in which the condenser has been adjusted to best conditions of resonance, and that a continuous current is employed, interrupted at least 50 times per second.

The tube, as received from the maker, will produce the ray vigorously for a time, but if the excitation be very powerful, it will shortly break down and cease to emit the ray. What is observed during the break-down is a bluish purple halo which makes its appearance at the cathode, and a halo at the anode as well, though this is grayish in tint. Gradually the blue halo lengthens out into a pencil, extending from the centre of the cathode towards the impact surface, and in a short time the pencil extends completely from the cathode to the impact plane. As soon as the blue line is seen to do this, the tube fails to emit the ray when viewed through the fluoroscope. Following this, the tube slowly fills with a bluish light to a greater or less extent. This "dead" period of formation will last for hours, the time being usually shortest in such tubes as are provided with a large anode surface, in addition to the metallic impact plane.[1] As the "formation" reaches completion, the fluorescence of the tube becomes a brilliant greenish-yellow. The halo about the anode disappears from its face, and is seen as a faint blue light in its rear; the pencil of blue light has also disappeared, though there may still be a faint halo in front of the cathode. During this period the resistance of the tube has greatly increased, it having fallen to a low value during the break-down. The fluoroscope now glows brilliantly, but the rays have scarcely any penetrating power—a hand placed in front shows only in outline, scarcely any details of the skeleton being visible.

A phenomenon is now marked in the focus tube, which persists throughout its life, and may be termed the impact plane of

1. "Röntgen Ray Tubes," by the author, *Electrical World*, Oct. 3, 1896, p. 383.

the ray. This is very clearly defined on the walls of the tube, and sharply marked on the screen of the fluoroscope. Its very sharpness of outline is an extremely significant fact which has so far received only the barest mention. Further than this, the rays given off in this plane exceed in intensity those given off along any other plane, and if we differentiate penetrating power from mere intensity, this property is also most marked in the impact plane; but until satisfactory radiometric methods are devised it is useless to attempt to state any ratios.

As the tube is continued in use it enhances, both in intensity of ray and penetrating power, the cathode and anode halos growing continually less perceptible.

After a time a period of decline sets in. The vacuum becomes extremely high, the fluorescence is scarcely perceptible, and the ray is weakly emitted, but its penetrating power has become so great that even the bony skeleton of the body becomes quite transparent. It is now customary at this stage to heat the tube with the flame of an alcohol lamp. As some erroneous statements have been made in this connection, the phenomena will be treated in detail. When the flame is first applied to the tube it darkens, being doubtless short-circuited by the aqueous vapor condensed on its surface. From time to time occasional flashes occur in the tube, but at length, with surprising suddenness, the entire bulb fills with the usual glow accompanying a highly active state, and the ray is emitted with great power. If at this juncture the heat is not very cautiously applied, the vacuum will be quickly lowered to the point of breaking-down; the impact plane will grow red hot, and the penetrating power of the ray is feeble. By deftly reversing the tube and manipulating the heating, the vacuum can be controlled within wide limits.

After a bulb reaches the point in use when heat must be applied, it ceases to be dependable, and its useful life is about over. Its uncertainty is greatly increased if the bulb has become blackened by a deposition of platinum, which rapidly occludes the remanant gases as they pass it in convection currents. To avoid this blackening, I have lately had the glass-blower substitute aluminium for the platinum of the impact plane. I have not found this change, so far, to affect the emission power of the tube, and besides, it lessens the cost. My experiments on comparative emission power of different substances have not been so extensive as could be desired; yet I have found that a surface of glass is best adapted for rays of the highest penetrating power.

In summarizing these experimental facts, is it not reasonable to assume that we may have some basis, some guidance, though slight, for an explanation of the Röntgen ray?

The first significant observation is the behavior of the tube as the blue pencil of light becomes evident. Vacuum tube experiments have demonstrated almost to a certainty that such phenomena are due to intermolecular collisions or impact; or, what

amounts to the same thing, that the mean free path of the molecules is short. As the vacuum lowers, this pencil grows in length and brightness, its molecular path, all the while, shortening. Conversely as the tube builds up, the molecular path increases, until at the moment the ray reappears, there is good evidence for believing it extends from the cathode to the impact surface. This, then, is the one prime condition for the production of the ray. At the same time, it seems equally clear that there is within the tube a certain critical pressure of its contained atmosphere, requisite for the generation of the ray. But the mean free path from cathode to impact plane, and the critical pressure, are correlative conditions. The suddenness with which the activity of the tube is resumed upon heating, is thus explained. The investigations of Rigi may be instanced in confirmation. He found that the critical pressure was some simple function of the distance between the cathode and impact plane.[1] But it has been found by many others that the shorter this path, the lower the working vacuum might be in the tube.

The second significant observation is the sharp definition of the impact plane on the walls of the tube and the screen, or exposed dry plate. This is indicative that the ray is generated at the surface of impact or in its immediate neighborhood. Experimental evidence all points to an impact surface for the production of the ray. If the focussing tube be accurately made, the principal impact is on the metallic anti-cathode; but whatever be the shape of the tube, there is more or less weak dispersion of the charged molecules of the cathode stream; and these impinging on almost the entire surface of the tube, constitute them a weak secondary source. Since the ray is produced only on impacted surfaces, and not in intermediate space, it follows that a mean free path is necessary to the impact surfaces.

Returning again to the principal impact surface, the usual pictorial representations of tubes are very inaccurate. The path of the cathode rays and the generated Röntgen rays is drawn as if it followed the law of incidence and reflection for transverse, or light waves. Here, again, is seen a further proof of the foregoing statements. That the law of incidence and reflection is not followed is because, at the surface of incidence, there occurs a transformation of energy resulting in the generation of a specific vibration.

Whatever be the substance of the impact surface, no true reflection occurs from it; and if it shuts off the rays in any direction, it does so by absorption. In this sense the material of the impact plane is without any influence on the distribution of the ray there generated. Another matter of interest is the change which occurs when the metallic impact grows red hot. In all my experiments, I have invariably found that the result is to

1. *Electrical World*, Sept. 26, 1896, p. 369.

lower the penetrating power of the rays. This may be due either to *decreased* amplitude or *increased* time of vibration, either or both being caused by the heating, which, driving off more occluded or impact gas molecules, and even the metallic molecules from the surface, lowers the vacuum; the electro-static capacity of the tube decreases, the voltage at its terminals lowers, and the molecules of gas are projected from the cathode with less velocity.

The question now arises, what is the hypothesis concerning the nature of these rays, which will best explain the observed facts here noted? The answer is beset on every hand with difficulties, but that which has best served to guide me has been one based on the vortex theory of atoms. But the difficulties here encountered are great. The vortex theory has only received a partial mathematical development, and scarcely anything has been so far possible towards physical demonstration.

In order to present these views an assumption will be made—that the active molecules involved are vortex filaments. Whether such filaments are vortex rings of matter in the ether, or be simply ether in rotation, involves difficulties whose solution has scarcely begun. We will assume also the former condition.

If such a vortex filament is to be endowed with the power of emitting the range of transverse vibrations which the light and heat spectra demand, let it further be granted that it may emit a certain range of longitudinal vibrations. If the ray suffer collision without great change of radius, the increase of its energy will give rise to transverse vibrations. This will account for the halos and pencil of blue light. If it be granted that intermolecular collisions of gaseous molecules do not result in great changes of radii, we have an explanation for the necessity of a mean free path from the cathode to the impact plane, as a condition requisite for the generation of the ray. Against such supposition may be opposed the results of mathematical analysis[1]

The effect of the impact of such a ring on a fixed surface is thus described by Lord Kelvin: "When a vortex ring is approaching a plane large in comparison to the dimensions of the ring, * * * it begins to expand, * * * and will expand out along the surface, losing in speed as it does so."[2] There will be a period then when the impact or translational energy is all absorbed in increase of radius; then will begin a series of harmonic vibrations of radial length, assuming the ring is not deformed circularly from the impact. What will be the resulting strain imparted to the ether? Would not a vibration rate be impressed upon the ether, and would not this be longitudinal? This would only be possible in case the ether can suffer longitudinal displacement.

1. "Motion of Vortex Rings," J. J. Thomson, Part II; also Part I, for the vibrations of such rings.
2. *Nature*, Vol. XXIV., p. 47.

What has been stated of the vibratory elastic properties of such vortex rings with respect to transverse vibrations, can be applied to longitudinal movement, that "the vibrations are influenced by the type of displacement and their restitution force, involving constants of space, time and mass.[1]"

In the light of our present knowledge, such an hypothesis furnishes the only rational explanation of the influence of the hardness of the impact surface, conditions of excitation of the tube being the same, upon the penetrating power of the ray. For a given rate of translation of vortex rings, the less yielding the impact surface, the greater would be the radial increase, or, what amounts to the same thing, for a given vibration frequency, the amplitude would be greater.

The modes of attack on the vital question, whether such rays are transverse or longitudinal, have been indirect ones. The question can not be settled until appealed to some such crucial test as spectrum analysis furnishes for transverse vibrations. Who is the genius who will construct for us a grating fitted to analyze longitudinal spectra, as Professor Rowland has so beautifully done for transverse vibrations?

MR. C. O. MAILLOUX:—Before the audience disperses, I think it is only proper that we should emphasize the fact that we have been favored in a signal manner this evening in the discussion of the all-absorbing topic which bears such an important relation to the latest advance in the science of physics. This evening we have had worthy representatives of all the large cities within a radius of three or four hundred miles. We have, to begin by the perhaps best known and most favorably known, Prof. Rowland, who represents one of the leading universities of the world, as the representative of Baltimore; Dr. Kennelly from Philadelphia, and our own and only Prof. Elihu Thomson from Boston. I think that it is meet and proper that I should propose a vote of thanks to the distinguished—I will not say foreigners—but the distinguished guests from afar—for they come to see us so seldom—for the honor and the pleasure which they have conferred upon us by coming to meet with us this evening to add the light of their experience and knowledge (theoretical and experimental) in connection with this important matter; in which vote of thanks I will include the distinguished home talent that has contributed to the enjoyment and enlightenment of the occasion, especially the representative from our own worthy Columbia University—Dr. Pupin.

THE CHAIRMAN:—All those in favor of this very graceful motion will please signify it by saying aye.

[The motion was carried.]

1. J. Clerk Maxwell, article on "The Atom," *Ency. Brit.*

[Communicated After Adjournment By Charles T. Rittenhouse.]

I am in thorough accord with the sentiments expressed by Dr. Kennelly in the course of his remarks on Wednesday evening last, to the effect that every endeavor should be made to co-ordinate the results of experiments with Röntgen rays with the generally accepted theories of light, rather than to propose new theories, concocted without mature deliberation, likely to be upset at any moment by the announcement of some new phenomenon. It seems to me that it would be preferable to withhold opinions regarding the exact nature of Röntgen rays until a sufficient number of facts have been collected to form a basis for the advancement of a new theory.

Allusion was made during the course of the discussion to the reflection of Röntgen rays, and apparently little credence is placed in the results of investigations thus far pursued in this direction. The presentation of a conclusive proof of regular reflection, although it would add little more to one theory than to another, still would be a most valuable contribution to our knowledge. Several investigators, both here and abroad announced in the early spring and since, that they had found Röntgen rays to be regularly reflected, but as many more contradicted these assertions so that the results as a whole have been discredited.

Had an opportunity offered itself it was my intention to bring before the members present, the results of some experiments, which although I do not consider absolutely conclusive, still indicate that regular reflection of Röntgen rays exists. These results I wish now to bring to the attention of the Institute, believing that they deserve more careful consideration than they have apparently received thus far. The experiments herein outlined were performed by Prof. O. N. Rood, of Columbia University, and described by him in *Science* of March 27th last, also before the Washington meeting of the National Academy of Sciences, and with important additions more recently in the *American Journal of Science*. In the second set of experiments plane metallic surfaces of both platinum and speculum metal were employed as reflectors, and the effect produced on a sensitive photographic plate by Röntgen rays compared with the effect obtained under the same conditions, but when using ordinary light. The method employed was as follows: Röntgen rays from a vacuum tube were allowed to fall upon a vertical plane metallic surface at an angle of approximately 45 degrees, thence reflected either regularly or by diffusion to a sensitive plate protected by a screen, found impervious to the direct action of sunlight after an exposure of eight hours, and also by a plate of aluminium. In front of the plate holder was placed a wire netting. Direct action between the tube and the sensitive plate was prevented by interposing a thick metallic screen. Owing to

the fact that the Röntgen rays emanated from the tube in a diverging beam, it follows that a large part of the reflecting surface should become a secondary source of radiation so that the entire surface of the sensitive plate should be affected. Thus the angle of incidence ranged from 30 to 60 degrees. As a consequence of the arrangement of the apparatus, the vertical wires of

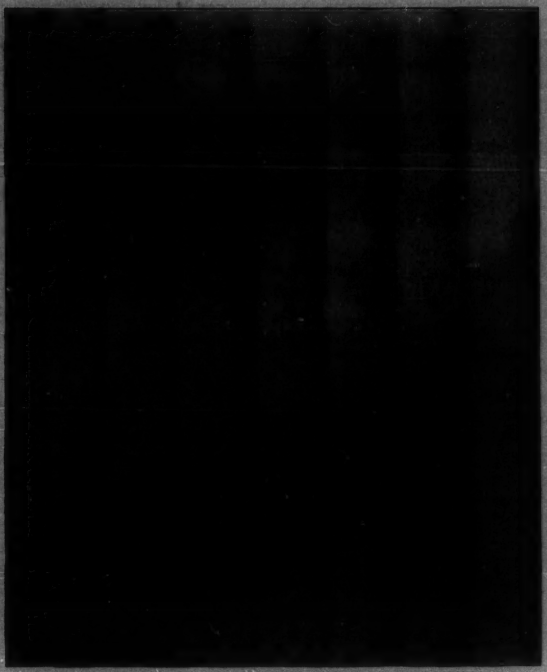


FIG. 5.

the netting produced a blurred image on the plate, due both to regularly reflected and diffused rays, while the horizontal wires produced a sharp image, due to the regularly reflected rays. Speculum metal although having a much smaller density than platinum, in these experiments gave the sharper horizontal lines. Professor Rood explained this fact as due to its smoother surface

and freedom from pimples. The prints thus obtained I have examined, but they are too faint to admit of reproduction.

The greenish light accompanying the Röntgen rays was now allowed to fall on a piece of white chalked paper that was substituted for the platinum or speculum mirror, its size and position corresponding with that of the mirrors. From the chalked

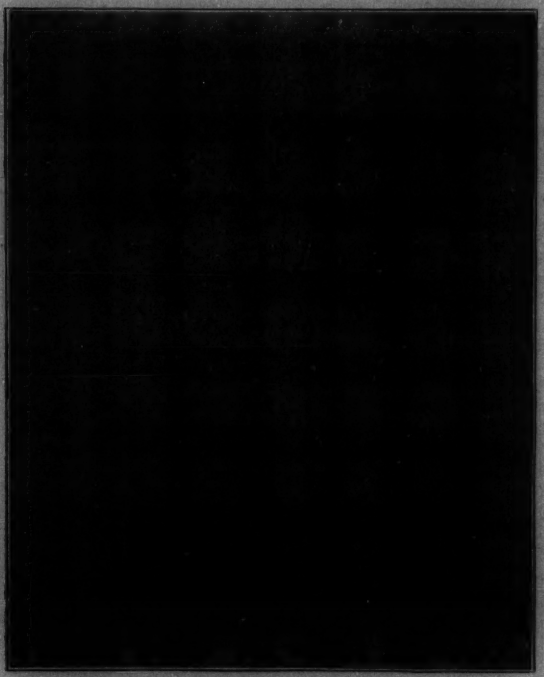


FIG. 6.

paper it reached the naked sensitive plate and there produced an image by strictly diffused light. This image was totally different in appearance from that obtained by the speculum mirror as it showed no trace of horizontal lines, they being replaced by broad streaks. It bore some resemblance to the Röntgen ray picture obtained from the platinum mirror, but still was different from

it. It is difficult to account for this result except on the supposition that a portion of the Röntgen rays had in the experiments with the mirrors undergone regular or speculum reflection.

In the third set of experiments a concave cylindrical mirror with a curvature not truly circular, was substituted for the plane mirrors which had been previously used, the object being to cause by specular reflection a beam of light to produce blurred or doubled shadows on the vertical wires of the grating, and at the same time give sharp single shadows of the horizontal wires. The relation between the ordinary Crookes tube employed and the cylindrical mirror and plate holder was such as to bring about this condition when ordinary light from the tube was used. If similar results could be obtained when Röntgen rays were substituted for ordinary light, it was highly probable that regular reflection of these rays exists. Fig. 5 is the result of the experiment. The horizontal lines although sharp as compared with the vertical lines, are accompanied by fainter parallel lines due to the Röntgen rays which have suffered diffusion; the vertical lines it will be noticed are broadened considerably. In sharp contrast with this figure, is Fig. 6 which was produced by the diffusion of ordinary light from a chalked surface under conditions exactly similar, with the exception that the sensitive plate was unprotected, to those present when Röntgen rays were used. Thus it will be seen that there are strong indications for believing that regular reflection of Röntgen rays exists.

In conclusion it may be said that when it is considered that Röntgen rays most likely consist of vibrations of such small wavelengths as to be comparable with the distances between the molecules of the most dense substances, it can scarcely be expected that regular reflection will take place to any large extent, but that dense substances having a most highly polished surface will still be as rough to these rays as a badly polished surface to ordinary light. In other words, regular reflection and diffusion are relative terms only, so that a given surface subjected to one mode of vibration behaves entirely different when receiving vibrations of a higher or lower order.

OBITUARY.

Henry Adams Craigin (Associate Member, June 6th, 1893, Member, May 15th, 1894,) was born at Boston, May 14th, 1867. He was connected with the Adams family of New England through Samuel Adams of Revolutionary fame, of whom he was a direct descendant.

He was graduated from the English High School of Boston, and entered the Massachusetts Institute of Technology in 1885, and was graduated in 1889, receiving the degree of Bachelor of Science in the course of mechanical engineering. During the years 1888 and 1889, in addition to his regular studies, he took the principal part of the course in electrical engineering, and his summer vacations were spent in connection with the United States Geological Survey, for which he was during the summer of 1889, assistant geologist.

In the fall of 1889 he entered the shops of the Union Pacific Railway Company, at Omaha, Neb., and, working through various branches, became Section Superintendent, and afterward Chief Inspector of Railway Supplies, and Purchasing Agent. During his service with the Union Pacific Railway Company he travelled extensively over the system of that, and other western railways for the purpose of collecting data. He resigned his position with the Union Pacific Railway in 1891, and spent the summer of 1891 in travelling over the Western States and Northern Mexico, studying railroad and other engineering problems, and inspecting a railroad in southern Texas for certain capitalists. During the years 1891 and 1892, he was Superintendent and General Manager of the Motor and Railway Company at Tacoma, Wash. He was also in the employ of the Portland Bridge and Iron Works, of Portland, Ore., for several months.

Electrical engineering had always been an attractive field to Mr. Craigin, and in the year 1892 he went to Pittsburgh in the employ of the Westinghouse Electric and Manufacturing Company. The success of his labors with the Westinghouse company amply proved his abilities in this direction.

In the year 1893 he was attached to the New York office of the Westinghouse company in the capacity of an electrical engineer, and was engaged in the selling and engineering branches of the business of that company at the time of his death, which occurred in the City of Mexico on November 27th, 1896. He had gone to Mexico partly for rest and partly in the interests of the company, but while there contracted a severe cold which resulted in pneumonia, the cause of his death.

Mr. Craigin was deeply interested in his professional work at all times, and his energy and perseverance had won for him the highest esteem of all his associates. His ability in electrical engineering was well recognized. He had devoted much time the last few months to the study of engineering problems involved in the proposed electric railway between Washington and Baltimore.

Outside of business, he was very much interested in music, and was well versed in English and French literature. He was fond of out-door athletics, and at the time of his death was a member of the Staten Island Cricket and Base Ball Club, the Boston Athletic Association, and was also a member of other social and literary organizations.

He was a man of the highest integrity and nobility of character, and by his death this INSTITUTE and the electrical profession are deprived of an able, energetic and valued member.

HARVEY L. LUFKIN (Associate Member June 17th, 1890) was born in Cleveland, Ohio, in 1857, and died in New York City December 21st, 1896, after an illness of but four days.

Mr. Lufkin had been engaged in electrical work for fifteen years. In 1886 he became associated with Messrs. Curtis, Crocker and Wheeler in establishing the C. & C. Electric Motor Company and played an important part in introducing and developing electric power. He was one of the first to operate electric motors on arc and incandescent lighting circuits, in spite of the strong opposition of the central station officers, who

looked upon this new application as a dangerous experiment. Fortunately he lived long enough to see electric power distribution become a very large and welcome part of the business of central stations. He was a pioneer in applying electric motors to the operation of factories and mills, and also had the satisfaction of witnessing the success and general acceptation of this improved method. Mr. Lufkin possessed the rare combination of technical knowledge with great business ability, enabling him to accomplish results which would be impossible for a man having either one of these qualities alone. This advantage, together with his excellent judgment, prevented him from making the mistakes which are so common in the development of a new art, and gave him a power to foresee the probable directions of successful progress, two examples of this prescience having already been noted. Although enthusiastic as to the value and possibilities of electrical applications, his good sense told him just where to stop, and it often happened that he would advise against the use of electrical apparatus when he did not think that it had decided advantages over other means, even though the electrical method might be perfectly feasible.

Mr. Lufkin was a very active member of the National Electric Light Association, and was a leading spirit in organizing and making a notable success of the Electrical Exhibition held in New York City during the spring of 1896.

Possessing an unusually bright and agreeable personality, he was universally popular among electrical men, and was sincerely liked by his friends and business associates.

His sudden death in the prime of his activity and usefulness is a great loss to the electrical industry, which always requires the services of men of exactly his stamp. F. B. C.

Books, etc., Received by the Institute.

Alternating Current and Alternating Current Machinery. By Dugald C. Jackson, C. E. and John Price Jackson, M. E., Members of the A. I. E. M. 729 pages, 325 cuts. The Macmillan Co., London and New York, 1896.

The Electric Pressure Developed by Alternators; Armature Windings for Alternators; Self-Induction and Capacity; Graphical and Analytical Methods of Solving Problems in Alternating-Current Circuits; The Magnetic Circuit of Alternators; Characteristics Regulations, etc; Regulation and Combined Output; Efficiencies, etc.; Mutual Induction; Operation of Ideal Transformers and Effect of Iron and Copper Losses; Efficiency and Losses in Transformers; Design of Transformers; Polyphase Conducting Systems and the Measurement of Power in Polyphase Circuits; Alternating-Current Motors; Polyphase Transformers. Appendices: A. The Application of Fourier's Series to Alternating-Current Curves; B. The Characteristic Features of Alternating-Current Curves; C. Oscillatory Discharges; D. Electrical Resonance.

American Telegraphy, Systems, Apparatus, Operation. By William Maver Jr., Member of American Institute of Electrical Engineers. 565 pages, 450 cuts. New York, Wm. Maver & Co., 27 Thames Street, 1897. Second Edition. Price, $3.50.

Contents.—Electrical Theories, Terms, etc.; Primary Batteries; The Dynamo Machine in Telegraphy; The Morse Telegraph System; Morse Telegraph Apparatus; The Condenser; The Rheostat; Galvanometers; Electrical Testing; The Delany Line Adjustment; Automatic Telegraph Repeaters; Duplex Telegraphy; The Quadruplex; Duplex and Quadruplex; Repeaters; Branch Office Signaling Devices; Loop Switches; Combination Duplex Systems, etc; Submarine Telegraphy; Automatic Telegraphy; Writing Telegraph Systems; Telegraphing from Moving Trains; Synchronous Multiplex Telegraphy; The Telephone; Simultaneous Telegraphy and Telephony; Varley-Athearn Diplex-Duplex; The Edison Phonoplex; Time Telegraph Service; Heliography; Military Telegraph Signaling; The American District Telegraph Messenger Service; Automatic Burglar Alarm Telegraphy; Printing Telegraphy; Fire Alarm Telegraphy; Police Signal Telegraph System; Railway Electric Block Signaling Systems, etc.; Overland Telegraph Wire; Underground Conduits; Underground and River and Harbor Telegraph Cables, etc.; Construction and Maintenance of Telegraph Lines; Specifications; Estimates; Miscellaneous; Tables.

The Mechanical and Other Properties of Iron and Steel in Connection with their Chemical Composition. By A. Vosmaer, Associate Member American Institute of Electrical Engineers, 203 pages, E. & F. N. Spon, New York and London, 1891.

Contents.—Influence upon Pig Iron, Cast Iron, Wrought Iron, and Steel of various metals, minerals, and glass, also Analyses of Iron and Steel.

Report of the Electrical Commission of Baltimore City, on a General Subway System, to be owned by the City of Baltimore. By Nicholas S. Hill, Jr., Associate Member A. I. E. E., Engineer of the Commission. 197 pages, 31 cuts, 7 maps.

DIED.

Leonard.—At Richmond, Va., February 6, 1897, Michael B. Leonard, Superintendent of Telegraph, Chesapeake and Ohio Railroad. Mr. Leonard was born in Philadelphia in 1855. He was elected an associate member of the Institute November 6, 1886, and transferred to membership May 1, 1888.

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